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DEVOTED TO

SCIENCE AND THE MECHANIC ARTS.

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PLANT ANALYSIS AS AN APPLIED SCIENCE.

BY HELEN C. DES. ABBOTT.

[*A Lecture delivered before the* FRANKLIN INSTITUTE, *January 17, 1887.*]

MISS ABBOTT was introduced by Prof. Persifor Frazer, and spoke as follows:

SCHLEIDEN,[1] in his principles of botany, states: "Botany is an indispensable branch of knowledge for the chemist and physiologist." I think he might have said, with equal truth, chemistry and physiology are indispensable branches of knowledge to the botanist. An acquaintance with these three branches of knowledge is indispensable to the plant chemist. If we consider that our food, fabrics, dye stuffs, perfumes, drugs, and beverages, are all derived from plants, we can scarcely fail to inquire into the functions and intimate structure of vegetable life The application

[1] *Principles of Scientific Botany.* By Dr. J. M. Schleiden, London, 1849.

of chemical knowledge to the study of plant life under all conditions is the first steps towards a practical solution of the problems of agriculture, materia medica, and the industries derived from plant sources.

As long ago as 1795,[1] a learned Scotch nobleman said: "Indeed there is no operation or process, in agriçulture, not merely mechanical that does not depend on chemistry." Fifteen years later, after Earl Dundonald's treatise, the first vegetable substance was accurately analyzed. Another period passed before the analyses of Liebig. Since that day investigators have been busily engaged in plant analysis.

Plant analysis to-day rests on a sure foundation as a distinct subdivision of general chemistry. Chemistry teaches us what vegetation needs for its growth, and points out the sources whence the materials for crops can be derived. Intense cultivation of the plant is the agricultural motto. The contrary is true for pharmacy. Plants which are to be used for medicinal purposes should grow under natural conditions. Cultivation of plants tends to diminish in quantity or to eradicate their noxious or medicinal principles. According to Prof. Vogel, hemlock does not yield coniine in Scotland, cinchona plants are nearly free from quinine when grown in hot houses, and tannin is also found in the greatest quantity in trees which have a direct supply of sunlight. Wild belladonna plants[2] contain more alkaloids than the cultivated.

Until within a comparatively very recent date, there were no schemes for vegetable analyses equivalent to *Fresenius's Manual for Inorganic Substances*. The irregularities of the methods of individual investigators in plant chemistry made it extremely difficult for students to follow this kind of analysis. The deficiency has been filled by the admirable book on *Plant Analysis*, by Prof. Dragendorff, of Dorpat, Russia. This book has appeared in a French translation,[3] and the first edition of an English transla-

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[1] *How Crops Grow*. By S. W. Johnson. London, 1869, p. 4.

[2] "The Alkaloidal Value of Cultivated and Wild Belladonna." By Girrard. *Pharm. Jour. and Trans.*, vol. xv, p. 153.

[3] *Encyclopédie Chimique*. Tome X. "Analyse chemique des Végétaux." Traduit de l'allemand et annoté. Par F. Schlagdenhauffen. Paris, 1885.

tion[1] was published a year before. Prof. Dragendorff does not claim to have written a perfect book. He offers a scheme, which, if followed, supplemented by well-known or original methods in the study of special or new compounds, will give the student a knowledge of the chemical constituents of a plant which he could not well obtain by a non-systematic scheme.

Dragendorff's scheme has been criticised as encouraging a mechanical method of work on the part of the analyst, but I think any student on working for the first time on a new drug by this method will find that he will be thrown very much on his own resources, and that the scheme serves him merely as a chain and anchor in a sea of novelty and uncertainty. It is indeed the most complete scheme for plant analysis which we have.

The scope of plant analysis is well outlined by Dragendorff in his introduction, and if my time permitted me, I could not do better than read it. The attention of the reader is directed to the great number of species of plants which occur in nature, to the great abundance and variety of their chemical constituents and to the circumstances that almost every skilful analysis of a plant that has not been examined yields new hitherto unknown products. The difficulties of plant analysis are pointed out, but it should be the effort of future investigators to endeavor to overcome these difficulties, when the importance of plant chemistry is considered in relation to scientific botany and chemistry, medicine, pharmacy, dietetics, agriculture, etc. This author says, that the analyses of plants in one respect possess an advantage over the analyses of minerals,[2] and in that respect can often be made more complete than that of a mineral.

It would not be possible within the space of an hour to give an accurate description of how to analyze a plant and the many methods which may be followed. I can give an idea of how to follow the scheme of which I have spoken as being the most complete, and the practical application of some facts derived from plant analysis.

The specimens which are presented for analysis should be in

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[1] *Plant Analysis.* By G. Dragendorff. Translated from the German by H. G. Greenish. London, 1884.

[2] *Plant Analysis.* English Translation, p. 2.

good condition and well selected as typical of the genus or species. In case of comparative studies the time of year of the gathering should be noted. All foreign substances and dust should be removed and care taken not to displace parts of the specimens.

All plants are chemically composed of two classes of substances and on incineration one class is decomposed into gases and the other class forms the ash constituents. These two divisions of the plant's constituents are known as the volatile and fixed parts. The manner of proceding with an analysis of a plant is somewhat different in the case of fresh plants and those which are air-dried. Fruits and succulent plants and fleshy roots may sometimes be examined with advantage in the fresh condition, especially if they contain much saccharine material or volatile products. Generally the parts of plants to be used for analyses are dried at a temperature under 30° C., or air-dried until in a state to powder; for all vegetable substances must be brought into fine subdivision before extraction, in order that the solvents may penetrate the cells.

The fine powdering of the material is of the utmost importance; a drug mill is usually used for this purpose. An agate or iron mortar may be used sometimes to advantage, or the material may be grated upon a fine grater, and then submitted to the same process of powdering and sifting, until it can be passed through a No. 80 sieve.

The Mexican ocotilla bark[1] is resinous and contains a wax, and it is very difficult to powder. From this fine powder the analysis yielded by cold maceration thirteen per cent. of waxy substance. Hot maceration gave nine per cent. An analysis from portions less finely powdered gave three per cent. less of wax. To estimate the amount of moisture retained in the air-dried plant, a small quantity of the powder from two to five grammes may be weighed and dried until constant weight at a temperature from 100° C. to 105° C. By means of this determination the results of all other estimations of the analysis can be calculated to the dry substance. Even in the case of fresh plants, it will be necessary for a quantitative examination of the entire plant at least to dry the portions which are to be treated with petroleum-ether, ether, and alcohol.

[1] "Preliminary Analysis of the Bark of *Fonquieria Splendens*." By Helen C. DeS. Abbott. *Proc. Amer. Ass. Adv. of Science*, vol. xxxiii. *American Journal of Pharmacy*, February, 1885

The powder, which has served for the moisture determination, is carefully burned at a dull red heat, and the ash residue weighed. This gives the total ash constituents of the plant. In many cases it is desirable to estimate the amount of soluble and insoluble ash, and to determine quantitatively one or more of the ash constituents, especially sulphuric and phosphoric acids and potash. In the ash may be found phosphorus, sulphur, silicon, chlorine, potassium, sodium, calcium, magnesium, iron, and manganese as well as oxygen, carbon, and nitrogen; rarely lithium, rubidium, iodine, bromine, fluorine, barium, copper, zinc, and titanium. The carbon, hydrogen, nitrogen, sulphur, and phosphorus are derived more especially from the organized parts of the plant, as the protoplasm and cell wall, and from carbonaceous substances, such as sugar, fats, and acids. It was stated that the volatile part of plants on incineration is gaseous, consisting principally of carbon-dioxide, watery vapor, and nitrogen. The inference being that the combustible portion of the plant contains the elements carbon, hydrogen, and nitrogen.

The fact that various mineral constituents are essential to the growth and development of plants is of practical value in agriculture. The soil must contain the various constituents in such quantity and form as to be available to the plant. The ash analysis of any plant indicates in a great measure the character of its surrounding soil, though the chemical composition in which the ash is contained in the plant is not necessarily the same as in the soil.

In investigating a new plant for the first time, all rational means for discovering its component parts should be resorted to. Before beginning the systematic analytical scheme, a micro-chemical investigation of thin sections of the plant, and even of the powdered plant may be followed. I have found it an excellent aid in the work, after knowing what constituents were present from chemical analysis, to determine in what tissues and cells these various substances are found. A drop of the extracts evaporated on a glass slide frequently indicates the character of the substances contained in them.

It is of importance to determine if volatile oils or acids, alkaloids and other substances are present, which can be separated by distillation, and for this purpose a sufficient quantity of the powdered plant may be mixed in a convenient vessel with water,

acidulated water, or milk of lime, and the mixture heated, preferably by steam. The distillate is condensed and may be examined as to its reaction, odor, and physical appearance. If the aqueous distillate is agitated with a light petroleum-ether[1], volatile products may be readily obtained.

Many volatile oils diffuse in moist air and pass off with the petroleum-ether, if precautions are not taken to prevent it, but a system by Osse[2] has been devised to evaporate the petroleum-ether and save the volatile oil.

Distillation of volatile principles may be sometimes substituted by other methods, such as "infusion" and "enfleurage," of which I shall speak later.

The following is the general plan I usually follow, based upon Dragendorff's scheme in order to determine the constituents of any plant. Twenty, fifty or 100 grammes of the dried powdered plant are weighed and macerated with successive solvents. The solvent is added in the proportion of ten c. c. to one gramme of powder. This is allowed to stand, with frequent shaking, for eight days, when the liquid is removed with a pipette or filtered from the powder. The residual powder is then rinsed with more of the solvent, which, added to the extract first obtained, is made to a known volume. The powder is dried at the ordinary temperature, and is then ready for maceration with a second solvent, and so on, until the sequence of solvents has removed all soluble matter from the powder. The residual insoluble portions are cellulose, lignin, and other allied substances, which form the firm frame work of the plant.

The solvents used must be chemically pure. The order of solvents recommended by Dragendorff, and the classes of compounds which may be extracted by them are given in the table.

PETROLEUM-ETHER EXTRACT.

Ethereal oils; volatile fat acids; glycerides; waxes; camphors; cholesterin or allied substances; chlorophyll and alkaloids with fixed oils; aldehydes; ethereal salts; alcohols; aromatic acids; resins.

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[1] Manufactured by Dr. H. W. Jayne, Frankford, Pa.

[2] *Archiv. d. Pharm.* (3), vii, 104 (1875). (*Year-Book Pharm.*, 1876, 362.)

ETHER EXTRACT.

Resins; waxes; fats; chlorophyll; coloring matters; organic acids; glucosides; alkaloids; (caoutchouc, chloroform, or bisulphide extracts).

ALCOHOL EXTRACT.

Tannic acids; bitter principles; organic acids; alkaloids; glucosides; glucose; saccharose; coloring matters; resins.

WATER EXTRACT.

Mucilaginous and albuminous substances; dextrin and other carbohydrates; saponin and allied compounds; glucoses; saccharoses; organic and mineral acids.

DILUTE SODA EXTRACT.

Metarabic acid; albuminous substances; phlobaphenes, etc.

DILUTE HYDROCHLORIC ACID EXTRACT.

Parabin; oxalate of calcium, etc.; starch.

DETERMINATION OF LIGNIN AND ALLIED SUBSTANCES AND OF CELLULOSE.

Benzole, chloroform, amyl alcohol, and acetic ether are frequently valuable solvents for certain extractions, although they are not included in the general scheme.

Dragendorff recommends the maceration to be conducted at the ordinary temperature, but a fixed oil, if present, may be extracted more readily by exhaustion at an elevated temperature. Such substances, as caoutchouc, may be readily extracted by boiling chloroform or bisulphide of carbon. If a known volume of the extract is evaporated, the residue will yield an approximate result of the amount of definite substances contained in the plant.

In my own work, I have usually found it convenient to take about twenty grammes of the powdered plant and exhaust them in a displacement apparatus. There are some advantages for this method in a preliminary study of the plant. The time necessary for the exhaustion is very much lessened; from ten to twelve hours at the most is ample time to allow the apparatus to run with each solvent, if the solvents are kept at a boiling heat during this period. It is a rapid way to determine qualitatively what constituents are to be found in any plant, and this may be followed by a careful

quantitative study on larger amounts. The general insight which can be obtained of the chemistry of a plant from this small quantity of material serves as a valuable guide for the future study on a larger scale.

The extracts obtained by heat show more proneness to oxidation than those from cold maceration, and there are some slight differences in the character of the extracts. The tendency of the higher temperature is to increase the number of constituents in the first extracts; *i. e.*, hot petroleum-ether will remove a considerable quantity of chlorophyll; hot ether will extract tannin, and hot alcohol extracts contain sugar, saponin, etc. After the hot alcoholic maceration; the water, dilute soda, and acid extractions are conducted at the ordinary temperature.

It will depend somewhat upon the object in view on the part of the analyst what course to follow in the study of a plant. If only one compound is to be isolated and examined disregarding the other constituents, suitable methods of study will be employed for this end. Even when Dragendorff's systematic scheme is followed, a fresh portion of powder should be extracted with water for an accurate estimation of soluble albuminoids, amides, and other classes of nitrogenous compounds. These subjects are very clearly stated in the volume of *Plant Analysis*, to which I have referred.

I wish to bring forward some well-known statements, which may serve to illustrate the practical application of facts discovered by plant analysis. One of the more recent applications of new processes to industrial chemistry is the manufacture of hop-resin extract [1] on a large scale. The use which is made of this extract, is in the manufacture of beers, and it is being used to a large extent in Philadelphia and New York, fully supplying the place of the ordinary hop. The process is somewhat as follows: The hops are loosely placed in large wire cages, and then are run into an immense boiler or "extractor." A heavy door is shut securely, and about 300 barrels of light petroleum are pumped in by an engine, and heat is applied by means of a steam coil, until a pressure of 100 pounds to the square inch has been obtained.

The object of this high pressure is to break or crush the glands, called lupulin, which contain the valuable principle, this being

[1] "Hop Extract." By W. B. Bissell. *Am. Jour. Pharm.*, April, 1885, p. 166.

taken up by the hot petroleum. The process is so managed that there is very little waste of menstruum, and the hop extract is readily separated; the petroleum-ether being used over and over again. One pound of this extract represents about twelve pounds of choice hops, and it has a great advantage over the hop itself, as it will keep for an indefinite time; whereas at the end of two years the hop is useless.

Hop-resin,[1] or bitter, was discovered from the chemical analysis of a plant, and it illustrates to what practical ends a fact derived from this source may be applied. The solubility of hop-resin in petroleum-ether is availed of also in the examination of beer.[2]

Vegetable wax is found on the surfaces of leaves, on the stem, and the berries of plants, and is obtained from many sources. The commercial supply comes from certain species of the palm tree family in considerable quantities. Carnauba wax is from a large Brazilian palm. *Myrica*, or myrtle, wax comes from the berries of an American and Mexican plant, *Myrica cerifera* of the *Myricaceæ* family, and Japan wax is obtained from *Rhus succedaneum*.

Vegetable wax[3] is principally used in the manufacture of candles, but on account of its greater dryness, it breaks much more readily than animal wax; hence, if animal wax is mixed in small proportions with vegetable wax, it answers very well. It is also used in adulteration of beeswax. Cerosin,[4] a wax from sugar cane is said to melt at 82° C. It has been proposed, on account of its high melting point, to use it in the manufacture of candles. Five hundred plants can furnish, it is claimed, one kilogramme of wax.

The bark of *Fouquieria splendens*,[5] or the ocotilla tree of

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[1] Lermer, *Vierteljahresschr. f. prakt. Pharm.*, xii, 504, 1863; Bissell, *Amer. Jour. Pharm.*, xlix, 582, 1877; Griessmayer, *Ber. d. d. Chem. Ges.*, xi, 292, 1878; Isleib, *Archiv. d. Pharm.* (3), xvi, 345, 1880; Cech., *Zeitschr. f. Anal. Chem.*, xx, 180, 1881.

[2] Griessmayer.

[3] *Matières Premières Organiques.* Par Pennetier, p. 771.

[4] *Matières Premières Organiques.* Par Pennetier, p. 771. *Annales de Chimie et de Physique*, lxxv, 218. *Annal d. Chem. und Pharm.*, xxxvii, 170, 1841. *Ibid* (new series), xiii, 451.

[5] *Proc. A. A. A. S.*, vol. xxxiii. *Amer. Jour. Phar.*, Feb., 1885. The analysis of this plant is among the first published accounts of plants treated by Dragendorff's scheme in this country.

Mexico, also yields a wax. The native Indians use this stem for illuminating purposes; it burns with a red, smoky, flame, and is called the candle tree.

The vegetable waxes are mixtures of resinous substances and the higher fatty acids, and differ from the fixed oils in containing in place of glycerin, cetyl, ceretyl, and myricyl alcohols; properly they contain ethers of higher alcohols of the ethylic series and free acids. The wax obtained from the *Gramineæ* or grasses, to which class sugar cane belongs, has been studied by König;[1] he found that it contained no glycerine but chloresterin, cerotic, palmitic, and oleic acids.

The importation of vegetable and mineral wax[2] for 1884, 617,992 pounds ($69,026); 1885, 1,056,438 pounds ($1,123,976).

The oils of vegetable origin used in commerce[3] are usually derived from grains; a few only are extracted from the fleshy parts of fruits. The oil is found in the form of minute drops in the rinds of fruits; the orange contains four different oils, and in many seeds the absence of starch is replaced by oil, and serves the future seedling for nutrition. The oil is usually obtained on a large scale by pressure, however oils are soluble in petroleum-ether, and may be extracted by it. In France,[4] the cultivation of oil-yielding plants occupied 445,000 hectares, the product of which amounted to 105,000,000 francs. Olive oil[5] is obtained from several species of the olive tree. It serves for many purposes, and enters into the food of some nations. In Spain, a kind of soup, made of oil, garlic and bread soaked in water, is eaten by the poorer classes.

The nuts[6] of *Carylus avellana* give an excellent table oil; it is also used in perfumery. The residue from the extract is used for almond confection, and is preferable to that made of ordinary almonds. A commerce is made in China of "Chou-lah[7]" obtained from one of the *Euphorbiaceæ*. This tallow is made into candles,

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[1] *Landw. Versuchsstat*, xiii, 241.

[2] Bureau of Statistics, Treasury Department, 1885.

[3] *Loc. cit.* Pennetier, p. 706.

[4] *Ibid*, p. 709.

[5] Pennetier, p. 709.

[6] Pennetier, p. 750.

[7] *Ibid*, p. 752.

which burn with a brilliant and white flame. There is an enormous demand for them. Many other plants of the same family furnish this oil. The genus *Bassia*, of the *Sapotaceæ* family, yield several important fats, among which is one known as Galam butter. This vegetable butter can replace animal fats, and is largely used in soap making. The annual report of the manufacturers of linseed oil alone for one year was figured at high rates, but the manufacture and uses of this oil are too well known to need more than a mention.

Olive oil in the *American Pharmacopœia* is replaced by cottonseed oil.[1]

The supply of cotton seed—*Gossypium*—is obtained from several countries, and may be said to be inexhaustible. The Southern States of North America contribute the largest quantity by millions of tons, a large proportion is not worth the expense of transit, and is burned for fuel and given to stock for litter. A considerable quantity is used in the manufacture of decorticated cotton cake and oil. Egypt is said to grow a superior quality of seed, and England derives her principal supply from there. Improvements in the method of irrigation are said to have increased the annual quantity, but the average of past years has been about 250,000 tons.

The seeds yield some twelve to twenty per cent. of oil. The oil in appearance, taste, and smell resembles fresh olive oil.

The fixed oils are chemically glycerides and are principally composed of glycerin, in combination with oleic, palmitic, and stearic acids. They are frequently solid at ordinary temperature, and their consistency depends upon the proportion of oleic acid present.

Commercial oils[2] frequently contain free acids, thus in palm oil the free acid calculated as palmitic acid usually varies from twelve to eighty per cent. The presence of free acid in an oil is doubtless the principal if not the only cause of its tendency to act on metals, and therefore seriously affects the suitability of an oil for use as a lubricant.

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[1] "Notes on Cotton-Seed Oil." By W. Gilmour. *Am. Jour. Pharm.*, Nov., 1885, p. 565.

[2] *Commercial Organic Analysis.* By A. H. Allen. Phila., 1887. Vol. ii, p. 28

Before leaving the subject of vegetable oils, I wish to call attention to the essential oil industry in Grasse.[1] The world-wide fame of this locality depends upon the essential oils of plants which grow wild or are cultivated in the neighborhood, The oil of lavender, rosemary, the garden thyme, of the *Labiatæ* family afford an important export industry of Grasse.

The following quantities of oil are delivered in Grasse every year: From the lavender, 80,000 to 100,000 kilogrammes; from thyme, 40,000, and from rosemary, 20,000 to 25,000 kilogrammes. The quantity sent out from Grasse probably meets the requirements of the whole world. Dalmatia only furnished the oil of rosemary and sends about 20,000 kilos of this essential oil into the market; Grasse also sends forth each year oil from the citrus species, especially oil of neroli which is much esteemed. Orris butter is distinguished above many other perfumes by an agreeable softness and great permanence. One of the houses in Grasse prepares four to ten kilogrammes yearly. Its value in Grasse is 1,500 to 1,800 francs the kilo. Besides the wholesale distillation of orange flowers and roses, some other aromatic plants are occasionally worked up when needed, though not to any great extent.

The processes used for extracting these perfumes by the methods of "infusion" and "enfleurage" are extremely interesting and may deserve a passing notice. The fat used as the basis of the "pommade" is selected from the best pig's lard or beef suet. The melting, its mechanical purification, and washing are conducted with great care. The stability of the fat is increased by its digestion with benzoin. The "infusion" is effected in large jacketted boilers, in which the fat is warmed by steam heat and the flowers are added. In the month of May over 10,000 kilos of rose or bigarade flowers pass daily for many successive days into the boilers of the factory of one house alone. The fat is diligently stirred by female workers, the expression by means of hydraulic presses is done by men. After the clearing of the fat, the finished "pommade" is at once weighed and stored in tin boxes.

In the case of the more delicate perfumes, the above method of "infusion à chaud" is replaced by "enfleurage." For this purpose light square wooden frames, about eighteen inches each way, in which a plate of glass can be placed, are used. Upon each glass

[1] F. A. Fluckiger, *Amer. Jour. Phar.*, March, 1886, p. 137.

is spread a quantity of fat in a thin layer, and this is strewn thickly with flowers. Sometimes contact with the fat is avoided, and the layer of fat is confined to the other glass wall of each compartment. When a perfumed oil is desired, cloths saturated with oil for the "enfleurage" may be used. The flowers are shut up in these glass compartments for a longer or shorter time, and are repeatedly renewed and replaced by fresh ones. The perfumed fat is mixed with alcohol by means of powerful stirrers. The alcohol takes up scarcely any of the fat, but the greater part of the odorous substances.

From several trials, I think, these processes of extraction may be applied to extract the delicate odors of barks, and other substances which would be destroyed by distillation, and have escaped detection up to this time.

Among the chemical substances recently introduced into the field of chemical industry[1] may be mentioned cholesterin, or lanolin, $C_{26}H_{44}O + H_2O$. Commercially, this substance is obtained from animal sources; but its wide distribution through the vegetable kingdom warrants its mention in this place. The singular property of this substance and its promising commercial future deserve more than a passing notice. Liebrich observed that cholesterin fat possesses the peculiar property of being able to absorb more than 100 per cent. of water, and this singular property was denominated by the great pharmacologist, lanosation, while the cholesterin, mixed with water, was termed by him, lanolin. He also first called attention to the great therapeutical value of lanolin, and shortly afterwards the industrial production of pure lanolin was commenced by a Berlin firm,[2] and its manufacture has been of late steadily increasing.

Lanolin is already taking the place of vaseline, paraffine, and lard. Its efficacy has already been established beyond doubt, and its superiority is due to the extraordinary readiness with which it is absorbed by the skin. This property is not known to belong in a similar degree to any other fatty substance. Besides the medicinal use it has also been already introduced into various branches

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[1] "Notes on Chem. Substances recently introduced into the Field of Chem. Industry." By J. Levinstein. *Jour. Soc. Chem. Industry*, Nov. 29, 1886.

[2] Messrs. Jaffé & Darmstadter.

of industry, such as perfumery, soaps, and pomades, also for greasing leather belting and for improving the pliability of leather.

The history of the wholesale drug trade for the year 1886 is one of the most remarkable on record since 1879–80. In a late number of the *Druggist's Circular*,[1] a summary is given of the year, from which I have taken some statements.

From the table of prices, as follows:

	January 1st.	*December 15th.*	*July 1st.*
Alcohol,	$2 10	$2 17	$2 09
Camphor,	23	22 25	23
Gum arabic,	70	95	82½
Morphine,	2 25	2 10	1 90
Vanilla bean,	10 00	18 00	12 00
Copaiba balsam,	34	36	34
Cubebs,	90	1 35	95
Tragacanth,	45	42	37½
Senna leaves,	15	27½	30
Golden seal,	14	18	13
Pink root,	35	47½	60

The advance in alcohol is said to be the result of a combination amongst the distillers. Balsam copaiba has for a long time been very scarce; but the arrival of new stocks will make it freer. "Cubebs, vanilla beans, gum arabic, tragacanth, senna, golden seal, serpentaria and pink root have been and are still very scarce and are likely to be higher." * * * "The largest movement in cocoa leaves ever known, took place early in the month." * * * "A short crop of senna coming at a time when all markets were poorly supplied and during an unusual active period, is responsible for the upward movement of the drug." * * * "The position of quinine just now is an interesting one, and the future of the market depends upon the source of barks, and that at present is expected to be upward, owing to reduced visible and prospective supplies."

The commercial value of these drugs depend upon certain chemical compounds which they contain. The scarcity of some of these drugs in itself is a sufficient inducement to push forward investigation in plant chemistry, and to endeavor to discover the same valuable constituents or their equivalent in new plants.

[1] *The Druggist's Circular and Chem. Gazette*, Jan., 1887.

The preparation of fine prescriptions has been advanced by the perfection in chemical methods of isolating plant constituents. The medicinal value of many drugs is due to one or more principles, and to be able to administer these principles apart from the accompanying compounds of the plant is a triumph of analytical skill.

A new and convenient form to prescribe the more important alkaloids, glucosides, and other active plant principles, is offered by Frederick Stearns & Co., Detroit, Mich. This firm manufactures alkametric granules and alkadermic pellets. These granules contain carefully prepared medicines representing the pure alkaloid or active principle.

The enormous quantity of drugs used to furnish alkaloids or other medicinal principles may be seen from the import[1] of cinchona bark or other barks used in the manufacture of quinine:

	Pounds.	*Value.*
1884,	2,588,307	$718,035
1885,	3,559,691	913,189

Of sulphate of quinia:

	Ounces.	*Value.*
1884,	1,263,732	$1,610,163
1885,	1,390,126	1,292,794

Other salts of quinia:

	Ounces.	*Value.*
1884,	712	$1,038
1885,	5,435	1,868

Cinchonidia:

	Ounces.	*Value.*
1884,	381,885	$206,405
1885,	478,747	220,846

A new York firm[2] has recently introduced upon the market quint-essential oils; the odorous principle of these oils is due to the stearoptens or camphors, which readily separate from the more volatile portions.

It has been suggested, owing to the scarcity of gum arabic to introduce upon the market a gum[3] from a Mexican tree, called

[1] Bureau of Statistics, Treas. Dept.

[2] Fritzsche Brothers.

[3] "Products of the Mesquite." By H. J. Schuchard. *Amer. Jour. Pharm.*, Nov., 1885, p. 542.

the mesquite. This gum exudes from the stem and branches during the summer months. The analysis of this gum offers several interesting features, amongst others its solutions can be combined with basic lead acetate and ferric salts without being precipitated, and it is suggested for this reason the more applicable in medicine than in gum arabic. It is probable that in time gum mesquite will become a commercial article of importance.

We are indebted to plants for our tea, coffee, and chocolate supply, and these articles may be reckoned among our foods; for one or all are used by every people.

In Spain, chocolate is looked upon as a necessity. The Spaniard may be seen making his early breakfast with a slice of bread spread with a thick paste of chocolate. The smiling-faced "El cocinero" told me how he prepared it, by carefully melting the solid chocolate cake to the desired consistency. A cup of steaming hot goat's milk is offered to the traveller to mix with this chocolate if he is unable to take it straight.

"When Cortez and the Spaniards entered the vast Empire of Montezuma, they found the use of cocoa or chocolate as a beverage common. The Emperor alone drank it flavored with vanilla from a golden cup."[1] The Spaniards very jealously guarded as a secret the mode of chocolate manufacture, and were able to retain the monopoly of the trade for many years.

Theobromine, caffeine, and theine are the alkaloids which give cocoa, coffee, and tea their exhilarating properties. They owe their aroma to certain volatile oil, which in the case of cocoa is probably developed by roasting.

Tea[2] is of the utmost importance as an article of consumption, and far exceeds in demand cocoa or coffee. Tea can be grown in a wide range of climate; in Pekin with winters of Russian severity to Canton and Macao. Any country having a long and hot summer and a cold winter can grow tea. The proportion per head of consumption for Great Britain and Ireland during 1875 was 4·44 pounds.

The very best workers in gathering the tea leaves rarely earn as much as six pence a day, and until other nations can raise tea

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[1] *Tropical Agriculture.* By P. L. Simmonds. London, 1877, p. 2.

[2] *Ibid*, p. 79.

for six cents a pound, they cannot compete with China in its production.

Guarana, a product allied to cocoa; maté, or Paraguay tea are also used. The same or allied alkaloids prevail in all the principal substances employed for these beverages in different parts of the world. After tea, there is scarcely any other staple of commerce used for dietetic beverages more generally acceptable with all classes than coffee.

The statistics of cocoa, tea, and coffee.[1]

IMPORTED INTO THE UNITED STATES.

	Pounds.	*Value.*
Tea:		
1884,	65,774,234	$13,504,798 56
1885,	69,820,172	13,725,380 75
Coffee:		
1884,	532,514,850	$49,685,689 30
1885,	572,222,841	46,723,290 16
Leaves and shells of crude cocoa:		
1884,	12,263,948	$1,673,088 00
1885,	10,300,078	1,332,375 00

The above facts, including the tables of statistics, show the extent of our dependence on the presence of chemical compounds in the various plant sources, from which we derive many of our supplies.

The consideration of the cereal products of the United States and our domestic sugar supply in relation to this subject, seems of sufficient importance to detain us for a few minutes.

"The total production[2] of the six principal cereal grains of the United States, for the census year, amounts to 2,697,962,456 bushels, an average of 58·8 bushels per head for the whole population. The total breadth of cultivation and the amount of product of each of the grains is as follows:

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[1] Bureau of Statistics, Treasury Department.

[2] *Report on the Cereal Production of the United States*, Dept. of the Interior, Census Office, 1884, p. 381.

Grain.	*Acres.*	*Production—Bushels.*
Corn,	62,368,869	1,754,861,535
Wheat,	35,430,052	459,479,505
Oats,	16,144,593	407,858,999
Barley,	1,997,717	44,113,495
Rye,	1,842,303	19,831,595
Buckwheat,	848,389	11,817,327
Total,	118,631,923	2,697,962,456

"Whether considered in respect to breadth of cultivation, total product, or average production per head of the whole population, these figures place the United States at the head of the grain-producing countries of the world." * * * "The tables of cereal production, taken in connection with the tables of other production, and these compared with the returns of previous census years, show that agriculture continues to be the leading productive industry of the country, and cereal production the most prominent feature of this industry. * * *

"The increase in grain production, since the previous census enumeration, is in part due to the cultivation of new lands in the West and in the Northwest, but more largely due to gain in farming regions already occupied in 1870. The popular belief that the chief increase in production and the rapid growth of the grain exports is due to the cropping of new and cheap lands, is not sustained by the census enumeration. The tables of production show that the most of the grain is in regions some time in cultivation and on lands ranging in value from $30 per acre upwards. * * *

"The actual production of 58·8 bushels per head of total population shows that the United States must be a grain-exporting country, notwithstanding the enormously large consumption by its population. The grain and flour exports[1] for the five years ending June 30, 1880, amount as follows:

Grain.	*Bushels.*
Wheat and corn,	833,692,207
Flour and corn meal,	24,850,316
Total value,	$892,788,117

"The profitable cultivation[2] of cereals on a large scale is more dependent upon climate than upon soil. Rocks of various geo-

[1] *Cereal Report*, p. 383.

[2] *Ibid*, p. 396.

logical ages underlie the different portions of the chief grain-producing regions. The immediate influence of the underlying rocks is, however, greater in the Southern and Western portions of the United States than in the Northern and Eastern." The production and distribution of grain in the United States is influenced largely by the physical character of the soil. "The portions producing the bulk of the grain have soils of reasonable fertility, but are also those which are easily tilled, and upon which the best machinery and labor-saving appliances can be most readily used."

"The acreage and crop[1] of wheat, in 1879, amounted to 35,430,-052 acres, 459,579,505 bushels, the acreage being 29·7 per cent. of all the land and cereals, and the product about 9·2 bushels per head of total population. * * * *

"There is but little wheat land east of the Hudson River, and although New York and Pennsylvania produce considerable wheat, the great bulk of the wheat country lies West of those states, beyond the seventy-seventh meridian and the Appalachian Chain of Mountains, and north of the Ohio River. * * * *

"The successful cultivation of wheat, in a commercial sense, is determined by a complicated set of conditions." In an agricultural sense, "the yield and quality of the crop practically depend upon but five conditions: The climate, the soil, the variety cultivated, the mode of cultivation, and the liability to destruction by insects." Chemistry has to do, however, with only the soil and the variety of grain related. The chemical composition of the grain and its value as a bread plant not only vary greatly in the different varieties, but also in the same variety from year to year, and on different soils.

Indian corn[2] stands first in amount of the cereal productions of the country. This cereal is more generally distributed over the country than any other; the place of its greatest production is on the fertile prairies and river bottoms of the West, and north of the thirty-sixth parallel of latitude. A comparatively few states[3] produce the bulk of the crop, the four states of Illinois, Iowa, Missouri, and Indiana producing upward of fifty-two per cent.

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[1] *Cereal Report*, p. 440–442.

[2] *Ibid*, p. 470.

[3] *Ibid*, p. 471.

"The chemical composition[1] of Indian corn varies more than wheat, as might be expected from the vast number and great difference of its varieties. As a whole, it is not quite so rich in albuminoids." It varies also much more in the amount of fibre. The average proportion of starch is less than in wheat, but the most noticeable difference is in the amount of oil. Indian corn, when in the "milk," is a most nutritious and excellent food "The chemical analysis of green corn shows respectively fourteen to fifteen per cent. albuminoids, * * * * an amount equal to that in the very best wheat flour."[2]

Oats[3] stand the third cereal of importance in the United States. Maine, Vermont, New York, and Wyoming raise more oats than any other cereal. The muscle-producing value of oats depends upon the amount of their albuminoids. The average composition of some American oats for analysis showed a higher percentage of albuminoids than the richest wheat flours. The amount of fat in oats ranges from four to nearly six per cent.

Barley[4] is successfully cultivated in a wider range of climate than any other cereal. It is the most hardy of all the cereals, and it grows in the north nearly to the point where all cultivation ceases. On the other hand, barley flourishes well in semi-tropical countries, and in this country the state of its greatest production is South. In Arizona and Nevada, more of barley than any other cereal was grown in the census year.

Rye[5] has become of very minor importance in the United States, in comparison with other cereals. It can be grown upon very poor soils. In Europe, for many ages, it was the principal bread-stuff of the people, for it could be cultivated on soils too poor to grow wheat. Pennsylvania has, at each census return, been the leading state in total production, now followed by New York.

From analyses, rye in the kernel is less nutritious than wheat, and the deficiencies in their respective flours is still greater. Wheat flours average about eleven per cent. of albuminoids, while

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[1] *Cereal Report*, p. 482.

[2] *Ibid*, p. 484.

[3] *Ibid*, p. 491.

[4] *Ibid*, p. 497.

[5] *Ibid*, p. 502.

rye flours average at about six per cent. On the other hand, rye bran is richer in albuminoids than wheat bran.

The popular belief that buckwheat[1] is less strengthening and more fattening than wheat, is founded on a chemical reason; for the percentage of albuminoids is low, ranging from four to eight per cent. The starch is in larger amount than in wheat, the percentage of oil being about the same. The peculiar aroma of buckwheat cakes is probably derived from the presence of an essential oil decomposed by heat.

Chemistry plays an important part in the cereal production of our country. The United States Agricultural Department furnishes several reports on this subject.[2] The analyses have been conducted to show the effect of evironment on the grain. The albuminoids, fats, and ash composition of American grain are given and compared with foreign crops, and the average composition of flour from different sections of the country has been studied.

The importance of chemical analyses in this connection is evident, for the relative chemical composition of a cereal decides its nutritive value, and this information is essential to the farmer in the selection of the kinds of grain for sowing. The percentage of chemical composition of grains varies from crops grown in different sections of the country, and furnish a scientific basis for careful selection of climate and soil.

Agricultural chemical analysis is usually conducted to show the aggregate percentages of groups of substances. All the nitrogenous compounds are determined together and classed as the albuminoids; starch, gum, sugar, and similar substances, as carbohydrates. Oils, waxes, and allied compounds are classed as fats. Special compounds existing in minute quantities, but belonging to one of these classes, would fail to be detected in such a general plan of analysis; such compounds might have great economic interest. Careful and detailed plant analysis can be the only means to discover and isolate these principles.

The source of sugar supply to the world are from a few plants; the beet, maple, sugar cane, and sorghum. In our country during

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[1] *Cereal Report*, p. 508.

[2] *Buls.*, No. 1, No. 4, No. 9, Chem. Div. Dept. of Agr. By Clifford Richardson.

1883-84 beet-sugar was all made at Alvarado[1], Cal. Sugar manufactured from the beet on the Pacific Coast is an assured success. The climate and soil of Northern California, Oregon and Washington Territory are especially suitable to this plant. A vast range of territory in our Northern States would be adapted for the cultivation of the sugar beet. The causes of past failures to establish a beet sugar industry may be remedied, depending upon more scientific methods of agriculture and chemical methods. Maple sugar is costly, the trees yielding this product are of slow growth, and their territory of cultivation limited. An adequate supply cannot be expected from this source, nor from the sugar cane of the South during the present stage of this industry.

If it is admitted that the prosperity of a country is shown by its advance in agriculture, then the onward march should be encouraged by every means in our power. We should look to our own acres for our sugar supply, since this can become practicable, and not abroad. The encouragement of a sugar industry in this country is of importance, when it is considered that over $100,000,000 is sent out of the country for raw sugar annually.

The problem of how to reduce our revenue does not apply to this industry, and in a recent letter on a plan of tariff revision, Mr. E. H Ammidown says: "Legislation to reduce the duty on sugar should be deferred until the conditions and prospects of the whole sugar industry have been more carefully investigated and are better understood. An industry which, if established, would produce $150,000,000 in value of a staple article of food required in every American household, and save $100,000,000 now or in the immediate future, annually paid to foreign producers, such an industry, with the example of France and Germany to encourage us, is of too serious importance to this nation to be treated by the national legislature otherwise than with the utmost caution and most cautious deliberation."

The following statistics will show the sugar and molasses importation:

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[1] "Our Sugar Supply." By H. W. Wiley. From *Bul.*, No. 2, Chem. Soc. of Washington. Jan., 1887.

For the year ending June 30, 1886,[1] free of duty from Sandwich Islands:

	Amount.	Value.
Molasses,	61,171 gallons.	$7,786 00
Sugar,	191,623,175 pounds.	9,166,826 00
Total,		$9,174,612 00

Dutiable.	Amount.	Value.
Molasses,	39,018,637 gallons.	$5,587,884 00
Sugar,	2,498,258,590 pounds.	71,606,918 00
Sugar candy, etc.,		23,333 00
Total,		$77,218,135 00
Value of all imported sugars and molasses,		$86,392,747 00
The value of all imported sugar and molasses, for the year ending June 30, 1885,		76,738,719 00
For the year ending June 30, 1884,		103,884,275 00
The total value[2] of domestic sugars and molasses amounted to		43,037,409 03
The amount of money sent out of the country during the last year to meet the demands of sugar consumption was		135,000,000 00[3]

The above figures show the amount of sugar and molasses consumed in the United States annually. If we are to obtain all of these products from our own lands, it is a reasonable question to ask, how is this to be accomplished?

Former analyses show that the yield of sugar from Louisiana cane is less than from cane grown in the tropics. The future prosperity of Louisiana growers need not suffer from this poorer juice. The recent experiments at Fort Scott[4] demonstrated that a given weight of cane, without notably increasing the cost of manufacture, yielded thirty per cent. more sugar than had ever been made before. The Southern sugar industry will thrive, with the encouragement of a greater sugar yield, and by the introduction of more scientific methods of growing and manufacture.

Of late years the manufacture of sugar from *Sorghum saccharatum* has attracted attention. So far, as a business project, it has proved a financial failure. From the recent chemical reports of the

[1] Bureau of Statistics, Treas. Dept. 1886.

[2] *Bul.*, No. 5, Chem. Div. Dept. of Agr., pp. 7 and 8.

[3] From. *Bul.*, No. 2, Chem. Soc. of Washington, p. 16.

[4] *Bul.*, No. 14, Chem. Div. Dept. of Agr., 1886. H. W Wiley, Chemist.

Agricultural Bureau, under proper conditions of cultivation, this cereal promises to become a profitable source of sugar supply.

I give a few of the chemical results of the late Fort Scott experiments.[1] Up to October 1st, the mean composition of the chips entering the diffusion battery was:

	Per Cent.
Sucrose,	8·76
Glucose,	3·28
Soluble solids,	14·88
Available sugar,	2·64

Following that date:

	Per Cent.
Sucrose,	7·02
Glucose,	4·16
Soluble solids,	14·89
Available sugar minus,	0·85

With such raw material it was found to be impossible to successfully manufacture sugar.

It must not be inferred from these discouraging analyses that sorghum is not capable of becoming a good sugar-producing plant. Many samples of cane brought fresh from the fields or from protected parts of piles of cane cut for a day, showed a remarkably high percentage of sugar.

On September 30th, a sample of cane from the carrier showed:

	Per Cent.
Sucrose,	12·39
Glucose,	3·76
Total solids,	17·8
Available sugar,	6·98

Such cane would yield 140 pounds of sugar per ton.

An October cane cut one day gave an average of 176·6 pounds of sugar per ton.

Dozens of samples of cane during the season would have given over 100 pounds of sugar per ton. When it is remembered that sorghum cane can be grown and delivered at the factory for $2 a ton, the importance of these figures cannot be over-estimated. If sorghum can be produced which will contain five per cent. available sugar from the whole crop, the future of the industry is a most promising one.

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[1] *Bul.*, No. 14.

Until the variations of the percentage of sucrose in the juice can be controlled, sorghum cannot be considered a profitable crop for sugar production.

It is purely a question of more scientific agriculture. So far as the processes are concerned, the problem of extracting the sugar from the cane has been solved.

To insure the financial success it will be important to select a suitable situation of climate and soil. Before embarking upon a large money outlay, the scientific representative of a company should experimentally grow, under trial conditions, sorghum cane in the localities where it is proposed to start the industry.

On a broad scale the northern and southern limits have been already defined. Seventy degrees Fahrenheit is the isotherm[1] for the best sorghum sugar production for June, July, and August; but cane for syrup will grow north of that line.

At a comparatively small expenditure the question of climate for special localities and other conditions could be tested by a chemical analysis of the plant, whose juices respond as quickly to favorable or adverse conditions as the mercury to heat and cold.

Dr. Wiley[2] recently, in his annual address as President of the Washington Chemical Society, said: "The hope of sorghum is not in new methods and new machinery, it is in the skill and patience of the agronomist. Wise selection of seed, intensive culture, judicious fertilization—these are the factors that can make the sorghum sufficiently saccharifacient."

It seems to me that the refinements of plant analysis are destined to play an important part in this connection. Chemical analysis of chosen seed would ensure a wise selection for planting. Analysis of the cane and juice would show the results of experimental culture. For experiment, the proportional constituents of the soil may be varied, to determine if the proportion of chemical constituents of the cane, detrimental or favorable to the production of richer juice, may be controlled.

Analyses would show what external chemical conditions are requisite to insure a vigorous growth, and if upon these depends a larger sugar yield. Series of experiments at different stages of

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[1] *Bul.*, No. 3, Chem. Div. Dept. of Agr.

[2] "Our Sugar Supply."

growth undertaken to discover the chemical processes attending growth, maturing, and ripening of the canes under trial conditions are necessary to be known by the chemical representative of the producer.

Plant chemistry in applying this knowledge to practical agricultural ends will fulfil a high aim. It may be suggested as a worthy object of agricultural experiment to discover what parts of the residual sorghum, juice and cane after the sugar extraction may serve a practical end. A profitable utilization of these products would assist the improved machinery and new chemical processes in lessening the cost of sugar production. Paper[1] has been manufactured from the cellulose of the sorghum cane. Future experiments will determine the separation and economic interest of other constituents.

Very many dye substances of vegetable origin are used industrially. It would detain us too long to enumerate the list, and I shall select a few of the well-known ones for illustration:

The dye-woods imported in a crude state are as follows:[2]

Camwood:

	Tons.	*Value.*
1884,	659·82	$65,461 00
1885,	730·	68,721 00

Fustic:

	Tons.	*Value.*
1884,	11,811·	$177,830 00
1885,	8,090·	119,689 00

Logwood:

	Tons.	*Value.*
1884,	55,921·59	$875,291 00
1885,	56,507·80	904,205 25

The madder plant was formerly grown to a large extent in many countries, and in France[3] large tracts of land were given up to its cultivation. "Madder[4] owes its importance to the beauty

[1] "Sorghum Saccharatum." By C. A. Goessmann. From *Trans. N. Y. State Agr. Soc.*, 1861. *Bul.*, xli, N. J. Agr. Experimental Station, 1887, p. 23. *Bul.*, No. 14, Chem. Div. Dept. of Agr., p. 56.

[2] Bureau of Statistics, Treas. Dept. 1885.

[3] *Tropical Agriculture.* By P. L. Simmonds. London, 1877, p. 369.

[4] *Hand-Book of Dyeing and Calico Printing.* By W. Crookes. London, 1874, p. 228.

and fastness of the tints it yields, and to the fact that by a variation of the mordants used, it produces rose pink, black, violet, lilac, and puce colors." The character of the soil where the madder grows affects the color of the dye. The roots grown in a rich clay soil exhibit a rose pink color; under other conditions, a deep red coloration.

Alizarin, the chief coloring matter of madder, is now produced artificially from coal tar in large quantities, though the madder is especially in request for woollen dyeing. This plant, which yielded such large revenues to the growers, is replaced by a cheaper manufactured product; very likely we should not have discovered the synthesis of its valuable dye, if our attention had not first been directed to it in the plant.

When it is remembered that coal tar is undoubtedly of vegetable origin, the many brilliant dyes derived from this source, are only evidences of what plant chemistry could have found in the carboniferous ages.

The following statistics show:

The amount[1] *of imported madder:*

	Pounds.	*Value.*
1884,	253,385	$13,521 00

Ground or prepared madder:

	Pounds.	*Value.*
1884,	1,458,313	$111,456 00
1885,	1,211,370	80,628 00

The natural or artificial alizarin:

	Pounds.	*Value.*
1884,	778,660	$296,123 00
1885,	1,470,864	404,002 00

Total value madder and alizarin:

1884,	$421,100 00
1885,	484,630 00

Many species of plants grown in different parts of the world, but especially the *Indigofera*, yield a glucoside called indican, which, under the influence of dilute mineral acids and certain ferments breaks up yielding indigo blue, and a substance resembling glucose.

"Indigo[2] has undoubtedly been known in Asia from a very

[1] Bureau of Statistics. 1885.

[2] *Hand-Book of Dyeing and Calico Printing.* W. Crookes, p. 447.

remote period of antiquity, since there exists in very ancient records, written in the Sanskrit language, descriptions of its mode of preparation mainly not different from the methods yet in use." The manner of cutting the plant and extracting the indigo is not the same in all countries. In India, the plants are grown from seed which are sown in the fall and spring, according to the kind of plant. As soon as the young plants are sufficiently forward they are replanted in regular rows. The flower buds are pulled off before they are fully developed, experience having taught that by so doing the leaves of the shrub become larger and yield more indigo, the coloring matter being chiefly present in the leaves.

The indigo of commerce is a blue dye stuff extracted by fermentation. Other plants[1] used occasionally for the extraction of indigo are more frequently employed directly in dyeing; they belong to the *Polygonaceæ* family. These plants are from India, China, Central Africa, and South America, and they can be acclimated in all warm countries. In the mode of indigo manufacture[2] two processes are employed. In the one the dry leaves are used, in the other the green leaves. This is the one in most common use. When the plant begins to flower it is cut down at about six inches from the ground and carried to the steeping vats with as little delay as possible, strewn horizontally into the vats, and pressed down by means of beams fixed into side posts, bamboo being placed under the beams. Water is immediately run in just sufficient to cover the plant. The pure water from the Ganges is especially sought for in these manufactories, and many indigo factories line the river banks. The time for steeping depends much on the temperature of the atmosphere, and can only be learned by experience and careful watching of the vats, but in close, sultry weather, with the thermometer at 96° in the shade, eleven or twelve hours are sufficient. In cooler weather more time is requisite.

When fermentation is established, the surface of the vat is covered with a violet scum. The liquid is drawn off through plug holes, in the wall of the vat. The fecula at the bottom is then removed to the boiler. It is brought to the boiling point as quickly as possible, and kept there for five or six hours. While boiling it is

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[1] *Matières Premières Organiques.* Par Pennetier, p. 513.

[2] *Matières Premières Organiques*, p. 516. *Bul. de la Société Industrielle de Mulhouse*, vol. xxviii, p. 307.

stirred to keep the indigo from burning and skimmed with a perforated ladle. When sufficiently boiled it is run off to the straining table, where it remains twelve or fifteen hours draining. It is then taken to the presses and gradually pressed. This process takes twelve hours. It is then ready to be taken out, cut, stamped, and laid in the drying house to dry.

In the manufacture of indigo the ordinary processes of fermentation, drawing-off the liquor, beating, and collecting the fecula, are generally well known and are followed with but trifling variation in different provinces and manufactories in India. The main points appear to be the watching and the soaking of the plant so as to be able to tap-off the infused liquid exactly at the right point of fermentation, and next to beat the liquid in the second vat long enough.

Indigotin as it is contained in the vegetable tissues is colorless, but it becomes blue on contact with air. If it is desired to change indigo blue to indigo white, it is only necessary to place it in the presence of a de-oxidizing and alkaline liquid, but as soon as air is admitted its blue color is resumed.

The dyeing of fabrics is based upon the transformation of indigo blue into soluble indigo white. The colorless matter is placed on the stuff, which becomes blue by exposure. The solubility of indigo in sulphuric acid is utilized for blue dyeing of wools.

Indigo has been made artificially by several methods, though the process so far is too expensive to manufacture the compound to replace the commercial supply from plants.

The table of statistics is as follows:

Amount of indigo[1] *imported:*

	Pounds.	Value.
1884,	3,074·48	$2,267,048 00
1885,	3,035,934·	2,007,066 00

Artificial indigo:

	Pounds.	Value.
1884,	None,	. . .
1885,	3,300·	$3,600 00

The dye commonly known as logwood has been cultivated in Jamaica[2] since 1715, and has been known and used in Europe from

Bureau of Statistics. 1885.

[2] *Crookes*, p. 342.

a short period after the discovery of America. The commercial supply of the dye is from *Hæmatoxylin campechianum*, a tree belonging to the natural order *Leguminosæ*. It is the wood of the tree which is used, and is met in commerce in the shape of large irregular blocks.

The only other tree besides logwood in which hæmatoxylin so far has been discovered, is the *Saraca indica* of the same natural order.

I stated[1] before the Academy of Natural Sciences, in November, the discovery of this principle in my analysis of the bark of the *Saraca indica*.

The *Saraca indica*[2] is called in India the asok or asoka tree, and it is said when this tree is in full blossom, there is nothing in the vegetable kingdom which affords a more beautiful object. Frequent mention is made of the plant in Hindoo mythology, and the bark is much used by native physicians in some diseases.

I undertook the analysis of this bark at the request of Messrs. Parke, Davis & Co., of Detroit, Mich., who liberally furnished me with a supply of the drug. The coloring principle exists in the bark in two or more conditions, as hæmatoxylin, and as oxidized products. The former was separated as yellow crystals, analogous in form to hæmatoxylin crystals from the true logwood The alcoholic extract of the bark contained about eighteen per cent. of a red-colored substance which agreed in color and dye tests with like constituents found in logwood. Mordanted cotton fabric was dyed with hæmatoxylin from Saraca bark, and presented the characteristic logwood dye colors.

The extracts of *Saraca indica* bark containing its coloring principle were tested with various reagents,[3] and it was observed that the reactions agreed with hæmatoxylin colors, and in no case with those of brazillin.

The bark of the commercial logwood tree is not used for extracting the dye, the wood of the tree being employed for this

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[1] "On Hæmatoxylin in the Bark of *Saraca Indica*." By Helen C. DeS. Abbott. *Proc. Acad. Nat. Sciences.* Phila., Nov. 30, 1886.

[2] *The Materia Medica of the Hindus.* By Udoy Chaud Dutt. Calcutta, 1877.

[3] S. P. Sadtler and W. L. Rowland. *Amer. Jour. Pharm.* Feb., 1881.

purpose. I determined the presence of a small quantity of hæmatoxylin in the logwood bark, and obtained with its extracts the same reaction without alkalies and other reagents as with the other wood extracts. But owing to the smaller percentage of dye in the bark of the specimens examined, the colors were less intense. In the case of the *Saraca indica* bark, the colors were very brilliant, and certainly indicated the presence of a large proportion of coloring matter in it. It would be of interest to secure specimens of the wood of *Saraca*, in order to determine if it contains the coloring principle, and should this be so, if it exists in sufficiently large quantities to warrant its introduction as a new source of this commercial product.

Last summer[1] I extracted from a Honduras plant, called "Chichipate," a yellow dye. It yielded with mordanted wool fabrics, colors resembling somewhat those yielded by fustic wood. A plant[2] was analyzed in the laboratory of Parke, Davis & Co., named *Cascara amargo*, from which a new alkaloid, pacramnine, was separated. This alkaloid is like berberine in its properties. Specimens of this plant were lately forwarded to me, and there is every indication of the relationship of identity of "Chichipate" and *Cascara amargo*. This incident is significant as deciding by means of chemical analysis the identity of plants under distinct names from different regions. No analysis under the name of "Chichipate" had ever been published until my own report. The dyeing property of the substance, chichipatin, separated from "Chichipate," I think is quite independent of the alkaloid, though berberine, it is well known, yields yellow colors with wool. I also separated a new camphor from this plant. It is crystalline, and under polarized light gives a beautiful play of colors.

During the year 1886, Prof. Trimble[3] separated a new crystalline camphor, phloxol from the underground portion of *Phlox carolina*. This substance resembles the camphor found in Chichipate. It is soluble in petroleum-ether, and this solvent is suggested

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[1] "Preliminary Analysis of a Honduras Plant, named 'Chichipate.'" Paper read before the A. A. A. S., at Buffalo, August, 1886.

[2] "Cascara Amargo." By F. A. Thompson. *The Ther. Gazette.* January 15, 1884, p. 8.

[3] "An Analysis of the Underground Portion of *Phlox Carolina*." By Henry Trimble. *Amer. Jour. Phar.* October, 1886, p. 479.

as a means of distinguishing powdered *Phlox carolina* from *Spigelia*. The latter contains no camphor. *Phlox* is frequently put on the market for *Spigelia*. The two drugs in the normal condition can be readily identified.

An estimate of the profitable ends of the chemical analysis of plants may be gathered from the above statements.

Plant analysis covers a wide field, for it includes the chemistry of the living and the dead plant. Its application to various industries is far-reaching.

Plant analysis in this country has been called an " infant industry." There are probably differences of opinion about the infant needing protection It certainly needs encouragement and support, when its importance as a citizen is recognized.

Plant chemistry should not only be directed towards the study of new plants ; but in the study of old plants it is to be encouraged ; for each new investigation of many well-known plants has revealed new chemical principles, and given additional knowledge of the old ones. We can never know to what practical uses the constituents of any plants may be brought, and the money value of this information should be considered.

Many chemical compounds, which are of the most practical use now made by synthesis, were first discovered in plants, products of living matter.

Synthetical chemistry has derived its knowledge from the results of analytical study. Researches in plant analysis have revealed many facts, though the exploration field is still wide.

In our present state of knowledge, plant chemistry is a safe political ground for either the Protectionist or Free-Trader. The vegetable cell has placed the tariff of human penetration so high, and protected so well its industry, that the plant enjoys the monopoly of proteids and a magazine of other substances. The Free-Trader may console himself, for if he is intelligent enough he can find out the processes, and start his own factory, duty free.

Prof. Cohn, of Breslau, tells us that it is only a question of time when we may hope for the chemist to succeed in doing what the simplest *Algæ* and mosses are able to do, namely, to produce starch from carbonic acid and water. On that day the bread problem, which is in fact the greatest of all social problems, will be solved.

Henry Draper Memorial.

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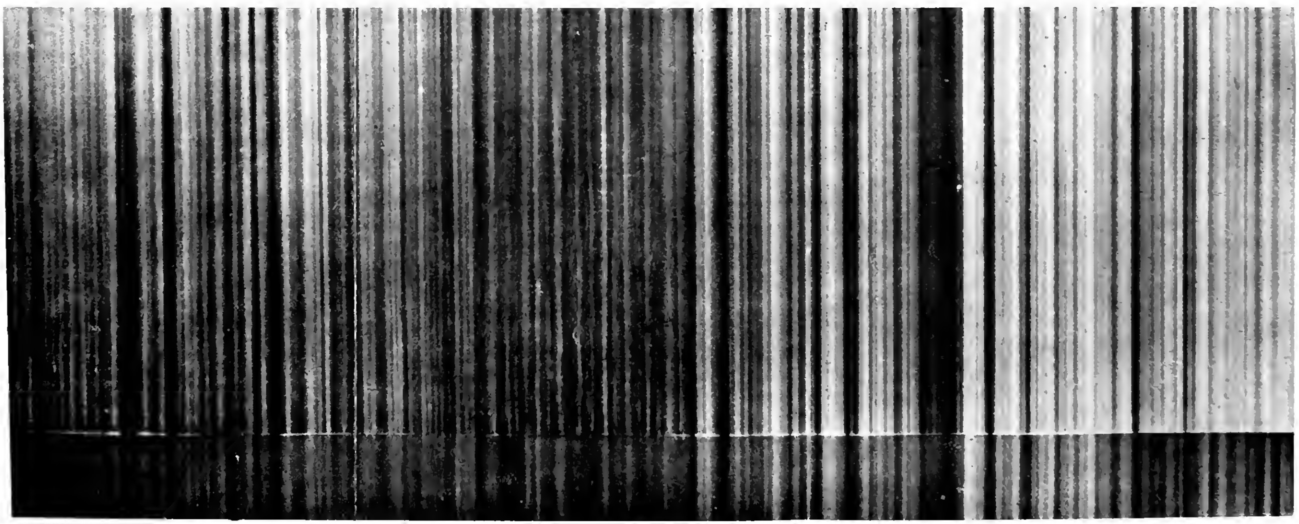

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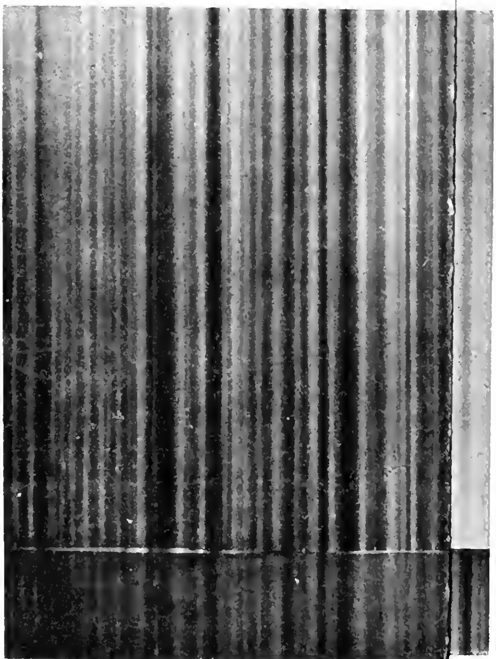

PHOTOTYPE

It is indeed true that those organic compounds, which are of the most importance in the life of the plant, the hydrocarbons, and the albuminoids are those which as yet have not permitted the secrets of their production to be discovered.

In the future, when synthesis has accomplished this prophecy and the synthetical chemist reigns supreme, our coming race to my imagination will be chemists, and our farmers will manufacture our food supply of proteids, sugars, and starch. The surface of the land will be one huge teeming laboratory. The plants, the analytical chemist and others of his race, asphyxiated by their environment, will have long ago passed away into a suffocating forgetfulness.

But, for the present, we must be satisfied to depend upon our humble colleagues, the plants, for our food and beverages; our fabrics, perfumes, and dye stuffs; our medicines, and other things too numerous to mention.

HENRY DRAPER MEMORIAL.

Dr. Henry Draper, in 1872, was the first to photograph the lines of a stellar spectrum. His investigation, pursued for many years with great skill and ingenuity, was most unfortunately interrupted in 1882 by his death. The recent advances in dry-plate photography have vastly increased our powers of dealing with this subject. Early in 1886, accordingly, Mrs. Draper made a liberal provision for carrying on this investigation at the Harvard College Observatory, as a memorial to her husband. The results attained are described below, and show that an opportunity is open for a very important and extensive investigation in this branch of astronomical physics. Mrs. Draper has accordingly decided greatly to extend the original plan of work, and to have it conducted on a scale suited to its importance. The attempt will be made to include all portions of the subject, so that the final results shall form a complete discussion of the constitution and conditions of the stars, as revealed by their spectra, so far as present scientific

methods permit. It is hoped that a greater advance will thus be made than if the subject were divided among several institutions, or, than, if a broader range of astronomical study were attempted. It is expected that a station to be established in the southern hemisphere will permit the work to be extended, so that a similar method of study may be applied to stars in all parts of the sky. The investigations already undertaken, and described below more in detail, include a catalogue of the spectra of all stars north of — 24° of the sixth magnitude and brighter, a more extensive catalogue of spectra of stars brighter than the eighth magnitude, and a detailed study of the spectra of the bright stars. This last will include a classification of the spectra, a determination of the wave-lengths of the lines, a comparison with terrestrial spectra, and an application of the results to the measurement of the approach and recession of the stars. A special photographic investigation will also be undertaken of the spectra of the banded stars, and of the ends of the spectra of the bright stars. The instruments employed are an eight inch Voigtländer photographic lens reground by Alvan Clark & Sons, and Dr. Draper's eleven-inch photographic lens, for which Mrs. Draper has provided a new mounting and observatory. The fifteen-inch refractor, belonging to the Harvard College Observatory, has also been employed in various experiments with a slit spectroscope, and is again being used as described below. Mrs. Draper has decided to send to Cambridge a twenty-eight-inch reflector and its mounting, and a fifteen-inch mirror, which is one of the most perfect reflectors constructed by Dr. Draper, and with which his photograph of the moon was taken. The first two instruments mentioned above have been kept at work during the first part of every clear night for several months. It is now intended that at least three telescopes shall be used during the whole night, until the work is interrupted by daylight.

The spectra have been produced by placing in front of the telescope a large prism, thus returning to the method originally employed by Fraunhofer in the first study of stellar spectra. Four 15° prisms have been constructed, the three largest having clear apertures of nearly eleven inches, and the fourth being somewhat smaller. The entire weight of these prisms exceeds 100 pounds, and they fill a brass cubical box a foot on each side. The spectrum

of a star formed by this apparatus is extremely narrow when the telescope is driven by clock-work in the usual way. A motion is accordingly given to the telescope slightly differing from that of the earth by means of a secondary clock controlling it electrically. The spectrum is thus spread into a band, having a width proportional to the time of exposure and to the rate of the controlling clock.

This band is generally not uniformly dense. It exhibits lines perpendicular to the refracting edge of the prism, such as are produced in the field of an ordinary spectroscope by particles of dust upon the slit. In the present case, these lines may be due to variations in the transparency of the air during the time of exposure, or to instrumental causes, such as irregular running of the driving clock, or slight changes in the motion of the telescope, resulting from the manner in which its polar axis is supported. These instrumental defects may be too small to be detected in ordinary micrometric or photographic observations, and still sufficient to affect the photographs just described.

A method of enlargement has been tried, which gives very satisfactory results, and removes the lines above mentioned as defects in the negatives. A cylindrical lens is placed close to the enlarging lens, with its axis parallel to the length of the spectrum. In the apparatus actually employed, the length of the spectrum, and with it the dispersion, is increased five times, while the breadth is made in all cases about four inches. The advantage of this arrangement is, that it greatly reduces the difficulty arising from the feeble light of the star. Until very lately, the spectra in the original negatives were made very narrow, since otherwise the intensity of the starlight would have been insufficient to produce the proper decomposition of the silver particles. The enlargement being made by daylight, the vast amount of energy then available is controlled by the original negative, the action of which may be compared to that of a telegraphic relay. The copies, therefore, represent many hundred times the original energy received from the stars. If care is not taken, the dust and irregularities of the film will give trouble, each foreign particle appearing as a fine spectral line.

Other methods of enlargement have been considered, and some of them tried, with the object of removing the irregularities of the

original spectra without introducing new defects. For instance, the sensitive plate may be moved during the enlargement in the direction of the spectral lines; a slit parallel to the lines may be used as the source of light, and the original negative separated by a small interval from the plate used for the copy; or, two cylindrical lenses may be used, with their axes perpendicular to each other. In some of these ways the lines due to dust might either be avoided or so much reduced in length as not to resemble the true lines of the spectrum.

The fifteen-inch refractor is now being used with a modification of the apparatus employed by Dr. Draper in his first experiments —a slit spectroscope from which the slit has been removed. A concave lens has been substituted for the collimator and slit, and, besides other advantages, a great saving in length is secured by this change. It is proposed to apply this method to the twenty-eight-inch reflector, thus utilizing its great power of gathering light.

The progress attained is best shown by the accompanying plate. *Fig. 1* is a direct copy of the spectra obtained in 1882. They were made by placing a 30° prism in front of a Voigtländer lens, having an aperture of two inches and a focal length of seven inches. The exposures lasted for about five minutes, and no clock-work was used. The instrument was directed successively to *α Lyræ*, *α Aquilæ*, *α Bootis*, and *β* and *γ* *Ursæ Minoris*, all of whose spectra appear in the figure. *Fig. 2* represents the spectra of *ζ Ursæ Majoris*, and of the adjacent fifth-magnitude star. It was taken with the eight-inch Voigtländer lens, with an exposure of five minutes, and illustrates the size of spectra used in preparing the catalogue of spectra of the brighter stars. One or two hundred of these spectra are sometimes photographed upon a single plate. *Fig. 3* was obtained with the Draper eleven-inch telescope, with two prisms. It represents the spectrum of *α Lyræ*, and was taken on November 5, 1886, with an exposure of fifty-nine minutes. *Fig. 4* was taken on January 21, 1887, in fifty minutes, with four prisms attached to the same instrument. It represents the spectrum of *β Geminorum*. All of these figures are reproduced directly from the original negatives, by the phototype process of F. Gutekunst, of Philadelphia. *Fig. 5* illustrates the method of enlargement described above. It represents the portions of *Fig. 4*

contained between the points marked *m* and *n*. The entire spectrum would, therefore, have a length nearly double that here represented. A less perfect spectrum of the same star was obtained on January 12, 1887. A portion of its enlargement is given in *Fig. 6*, adjacent to *Fig. 5*. Nearly all the lines of *Fig. 5* are shown less clearly in *Fig. 6*. Some of the remainder are due to the irregularities of the film described above, but they are probably too minute to be visible in the paper prints.

The results to be derived from the large number of photographs already obtained can only be stated after a long series of measurements and a careful reduction and discussion of them. An inspection of the plates, however, shows some points of interest. A photograph of *α Cygni*, taken November 26, 1886, shows that the *H* line is double, its two components having a difference in wave-length of about one ten-millionth of a millimetre. A photograph of *ο Ceti* shows that the lines *G* and *h* are bright, as are also four of the ultra-violet lines characteristic of spectra of the first type. The *H* and *K* lines in this spectrum are dark, showing that they probably do not belong to that series of lines. The star near χ^1 *Orionis*, discovered by Gore in December, 1885, gives a similar spectrum, which affords additional evidence that it is a variable of the same class as *ο Ceti*. Spectra of *Sirius* show a large number of faint lines besides the well-known broad lines.

The dispersion employed in any normal map of the spectrum may be expressed by its scale; that is, by the ratio of the wave-length as represented to the actual wave-length. It will be more convenient to divide these ratios by 1,000,000, to avoid the large numbers otherwise involved. If one-millionth of a millimetre is taken as the unit of wave-length, the length of this unit on the map in millimetres will give the same measure of the dispersion as that just described. When the map is not normal, the dispersion, of course, varies in different parts. It increases rapidly towards the violet end when the spectrum is formed by a prism. Accordingly, in this case the dispersion given will be that of the point whose wave-length is 400. This point lies near the middle of the photographic spectrum when a prism is used, and is not far from the *H* line. The dispersion may accordingly be found with sufficient accuracy by measuring the interval between the *H* and *K* lines, and dividing the result in millimetres by 3·4, since the

difference in their wave-lengths equals this quantity. The following examples serve to illustrate the dispersion expressed in this way: Angström, Cornu, 10; Draper, photograph of normal solar spectrum, 3·1 and 5·2; Rowland, 23, 33 and 46; Draper, stellar spectra, 0·16; Huggins, 0·1. *Fig. 1*, 0·06; *Fig. 2*, 0·10; *Fig. 3*, 0·63; *Fig. 4*, 1·3; *Figs.* 5 and *6*, 6·5.

The most rapid plates are needed in this work, other considerations being generally of less importance. Accordingly, the Allen and Rowell Extra-Quick plates have been used until recently. It was found, however, that they were surpassed by the Seed Plates No. 21, which were accordingly substituted for them early in December. Recognizing the importance of supplying this demand for the most sensitive plates possible, the Seed Company has recently succeeded in making still more sensitive plates, which we are now using. The limit does not seem to be reached even yet. Plates could easily be handled if the sensitiveness were increased ten-fold. A vast increase in the results may be anticipated with each improvement of the plates in this respect. Apparatus for testing plates, which is believed to be much more accurate than that ordinarily employed, is in course of preparation. It is expected that a very precise determination will be made of the rapidity of the plates employed. Makers of very rapid plates are invited to send specimens for trial.

The photographic work has been done by Mr. W. P. Gerrish, who has also rendered important assistance in other parts of the investigation. He has shown great skill in various experiments which have been tried, and in the use of various novel and delicate instruments. Many of the experimental difficulties could not have been overcome but for the untiring skill and perseverance of Mr. George B. Clark, of the firm of Alvan Clark & Sons, by whom all the large instruments have been constructed.

The progress of the various investigations which are to form a part of this work is given below:

(1.) *Catalogue of Spectra of Bright Stars.*—This is a continuation of the work undertaken with the aid of an appropriation from the Bache Fund, and described in the *Memoirs of the American Academy*, vol. xi, p. 210. The eight-inch telescope is used, each photograph covering a region of 10° square. The exposures for equatorial stars last for five minutes, and the rate of the clock is

such that the spectra have a width of about 0·1 cm. The length of the spectra is about 1·2 cm. for the brighter, and 0·6 cm. for the fainter stars. The dispersion on the scale proposed above is 0·1. The spectra of all stars of the sixth magnitude and brighter will generally be found upon these plates, except in the case of red stars. Many fainter blue stars also appear. Three or four exposures are made upon a single plate. The entire sky north of — 24° would be covered twice, according to this plan, with 180 plates and 690 exposures. It is found preferable in some cases to make only two exposures; and when the plate appears to be a poor one, the work is repeated. The number of plates is therefore increased. Last summer, the plates appeared to be giving poor results. Dust on the prisms seemed to be the explanation of this difficulty. Many regions were re-observed on this account. The first cycle, covering the entire sky from zero to twenty-four hours of right ascension, has been completed. The work will be finished during the coming year by a second cycle of observations, which has already been begun. The first cycle contains 257 plates, all of which have been measured, and a large part of the reduction completed; 8,313 spectra have been measured on them, nearly all of which have been identified, and the places of a greater portion of the stars brought forward to the year 1900, and entered in catalogue form. In the second cycle, sixty-four plates have been taken, and about as many more will be required. Fifty-one plates have been measured and identified, including 2,974 spectra. A study of the photographic brightness and distribution of the light in the spectra will also be made.

The results will be published in the form of a catalogue resembling the Photo-metric Catalogue given in vol. xiv, of the *Annals of Harvard College Observatory*. It will contain the approximate place of each star for 1900, its designation, the character of the spectrum as derived from each of the plates in which it was photographed, the references to these plates, and the photographic brightness of the star.

(2.) *Catalogue of Spectra of Faint Stars.*—This work resembles the preceding, but is much more extensive. The same instrument is used, but each region has an exposure of an hour, the rate of the clock being such that the width of the spectrum will be as before 0·1 cm. Many stars of the ninth magnitude will thus be

included, and nearly all brighter than the eighth. In one case, over 300 spectra are shown on a single plate. This work has been carried on only in the intervals when the telescope was not needed for other purposes. Ninety-nine plates have, however, been obtained, and on these 4,442 spectra have been measured. It is proposed to complete the equatorial zones first, gradually extending the work northward. In all, 15,729 spectra of bright and faint stars have been measured.

(3.) *Detailed Study of the Spectra of the Brighter Stars.*—This work has been carried on with the eleven-inch photographic telescope used by Dr. Draper in his later researches. A wooden observatory was constructed about twenty feet square. This was surmounted by a dome having a clear diameter of eighteen feet on the inside. The dome had a wooden frame, sheathed and covered with canvas. It rested on eight cast-iron wheels, and was easily moved by hand, the power being directly applied. Work was begun upon it in June, and the first observations were made with the telescope in October. Two prisms were formed by splitting a thick plate of glass diagonally. These gave such good results that two others were made in the same way, and the entire battery of four prisms is ordinarily used. The safety and convenience of handling the prisms is greatly increased by placing them in square brass boxes, each of which slides into place like a drawer. Any combination of the prisms may thus be employed. As is usual in such an investigation, a great variety of difficulties have been encountered, and the most important of them have now been overcome.

(4.) *Faint Stellar Spectra.*—The twenty-eight-inch reflector will be used for the study of the spectra of the faint stars, and also for the fainter portions near the ends of the spectra of the brighter stars. The form of spectroscope mentioned above, in which the collimator and slit are replaced by a concave lens, will be tried. The objects to be examined are, first, the stars known to be variable, with the expectation that some evidence may be afforded of the cause of the variation. The stars whose spectrum is known to be banded, to contain bright lines, or to be peculiar in other respects, will also be examined systematically. Experiments will also be tried with orthochromatic plates and the use of a colored absorbing medium, in order to photograph the red portions of the

spectra of the bright stars. Quartz will also be tried to extend the images towards the ultra-violet.

(5.) *Absorption Spectra.*—The ordinary form of comparison spectrum cannot be employed on account of the absence of a slit. The most promising method of determining the wave-lengths of the stellar spectra, is to interpose some absorbent medium. Experiments are in progress with hyponitric fumes and other substances. A tank containing one of these materials is interposed, and the spectra photographed through it. The stellar spectra will then be traversed by lines resulting from the absorption of the media, thus interposed, and, after their wave-lengths are once determined, they serve as a precise standard to which the stellar lines may be referred. The absorption lines of the terrestrial atmosphere would form the best standard for this purpose if those which are sufficiently fine can be photographed.

(6.) *Wave-Lengths.*—The determination of the wave-lengths of the lines in the stellar spectra will form an important part of the work which has not yet been begun. The approximate wave-lengths can readily be found from a comparison with the solar spectrum, a sufficient number of solar lines being present in most stellar spectra. As a difference of one ten-millionth of a millimetre in wave-length exceeds half a millimetre in *Figs. 5* and *6* of the accompanying plate, the readings may be made with considerable accuracy by a simple inspection. For greater precision, special precautions are necessary on account of the deviation caused by the approach and recession of the stars. The deviation found by Dr. Huggins in the case of *Sirius* would correspond to a change in the position of the lines of *Figs. 5* and *6* of about half a millimetre. If, then, satisfactory results are obtained in the preceding investigation, the motion of the stars can probably be determined with a high degree of precision. The identification of the lines with those of terrestrial substances will, of course, form a part of the work, but the details will be considered subsequently.

From the above statement, it will be seen that photographic apparatus has been furnished on a scale unequalled elsewhere. But what is more important, Mrs. Draper has not only provided the means for keeping these instruments actively employed, several of them during the whole of every clear night, but also of reducing the results by a considerable force of computers, and of publishing

them in a suitable form. A field of work of great extent and promise is open, and there seems to be an opportunity to erect to the name of Dr. Henry Draper a memorial such as heretofore no astronomer has received. One cannot but hope that such an example may be imitated in other departments of astronomy, and that hereafter other names may be commemorated, not by a needless duplication of unsupported observatories, but by the more lasting monuments of useful work accomplished.

EDWARD C. PICKERING,
Director of Harvard College Observatory.

Cambridge, Mass., U. S. A., March 1, 1887.

THE USE OF OIL FOR STILLING WAVES: WITH A DESCRIPTION OF A NEW OIL DISTRIBUTOR FOR THE USE OF MARINERS.

BY T. F. TOWNSEND, of Philadelphia.

[*A Paper read before the* FRANKLIN INSTITUTE, *at the Stated Meeting held Wednesday, April 20, 1887.*]

JOSEPH M. WILSON, President, in the Chair.

MR. TOWNSEND: The stilling of waves by means of oil, was known to the Ancients, and is mentioned in the writings of Plutarch, Pliny and Aristotle.

Its application for quieting rough waters, has long been practiced by whalemen, fishermen, and divers in their avocations, but its more general use, continuously applied in small quantities, during severe storms, by merchantmen, for securing their comfort and safety, is of comparatively recent date.

Had there been no potency in "pouring oil on troubled waters," it is hardly likely that the saying would have become so well known, and so often used as a comparison. The origin of the maxim I am unable to give, as I have never seen it used, except as a quotation.

Owing to so many recent publications of the experience of those who have tested the action of oil on high seas, the public is becoming much interested in the subject, and is curious to know

if it is a fact, that so simple a means will produce such decided results.

If we can believe the evidence of the many competent judges, who have made practical experiments, its efficacy is established almost beyond a doubt.

The subject possesses also much interest from a scientific point of view.

Franklin, who was ever on the alert for those things which escape the observation of ordinary persons, did not fail to notice the peculiar effect of oil on water, and his philosophical mind did not rest until he had investigated the subject.

Some of the results of those investigations are embodied in the following extracts of papers read before the Royal Society, June 2, 1774. They are so thorough and clear, as to cause and effect, that I cannot do better justice to the subject, than to reproduce them before the Society bearing his honored name.

Extract of a letter from the Rev. Mr. Farish to Dr. Brownrigg:

I sometime ago met with Dr. Dun, who surprised me with an account of an experiment you had tried upon the Derwent Water, in company with Sir John Prindle and Dr. Franklin. According to his representations, the water, which had been in great agitation before, was instantly calmed upon pouring in only a very small quantity of oil, and that to so great a distance round the boat as seemed incredible.

I have since had the same accounts from others, but I suspect all of a little exaggeration.

Pliny mentions this property of oil as known particularly to the divers, who made use of it in his days, in order to have a more steady light at the bottom.

The sailors, I have been told, have observed something of the same kind in our days, that the water is always remarkably smoother in the wake of a ship that has been newly tallowed than it is in one that is foul.

Mr. Pennant also mentions an observation of the like nature made by the seal catchers in Scotland.* When these animals are devouring a very oily fish, which they always do under water, the waves above are observed to be remarkably smooth, and by this mark the fisherman know where to look for them.

Old Pliny does not usually meet with all the credit I am inclined to think he deserves. I shall be glad to have an authentic account of the Keswick experiment, and, if it comes up to the representations that have been made of it, I shall not much hesitate to believe the old gentleman in another more wonderful phenomenon he relates, of stilling a *tempest* only by throwing up a little vinegar into the air.

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* *Brit. Zool.*, vol. iv, article "Seal."

Extract of a letter to Dr. Brownrigg, from Dr. Franklin, dated London, 7 November, 1773.

Dear Sir :—I thank you for the remarks of your learned friend at Carlisle. I had, when a youth, read and smiled at Pliny's account of a practice among seamen of his time, to still the waves in a storm by pouring oil into the sea, which he mentions, as well as the use made of oil by the divers; but the stilling a tempest by throwing vinegar into the air had escaped me. I think, with your friend, that it has been of late too much the mode to slight the learning of the Ancients. The learned, too, are apt to slight too much the knowledge of the vulgar.

The cooling by evaporation was long an instance of the latter. This art of smoothing the waves by oil is an instance of both.

* * * * * * * * *

In 1757, being at sea in a fleet of ninety-six sail bound against Louisbourg, I observed the wakes of two of the ships to be remarkably smooth, while all the others were ruffled by the wind, which blew fresh. Being puzzled with the differing appearance, I at last pointed it out to our captain, and asked him the meaning of it. "The cooks," says he, "have, I suppose, been just emptying their greasy water through the scuppers, which has greased the sides of those ships a little." And this answer he gave me with an air of some little contempt, as to a person ignorant of what everybody else knew. In my own mind, I at first slighted his solution, though I was not able to think of another; but recollecting what I had formerly read in Pliny, I resolved to make some experiment of the effect of oil on water, when I should have opportunity.

* * * * * * * * *

An old sea captain told me he had heard it was a practice with the fishermen of Lisbon, when about to return into the river (if they saw before them too great a surf upon the bar, which they apprehended might fill their boats in passing) to empty a bottle or two of oil into the sea, which would suppress the breakers, and allow them to pass safely.

A confirmation of this I have not since had an opportunity of obtaining; but discoursing of it with another person, who had often been in the Mediterranean, I was informed that the divers there, who, when under water, in their business, need light, which the curling of the surface interrupts by refractions of so many little waves, let a small quantity of oil now and then out of their mouths, which, rising to the surface smooths it, and permits the light to come down to them. All these informations I at times revolved in my mind, and wondered to find no mention of them in our books of experimental philosophy.

At length being at Clapham, where there is, on the common, a large pond, which I observed one day to be very rough with the wind, I fetched out a cruet of oil, and dropped a little of it on the water. I saw it spread itself with surprising swiftness upon the surface, but the effect of smoothing the waves was not produced; for I had applied it first on the leeward side of the pond where the waves were largest, and the wind drove my oil back upon the shore.

I then went to the windward side, where they began to form; and there the oil, though not more than a teaspoonful, produced an instant calm over a space several yards square, which spread amazingly, and extended itself gradually till it reached the lee side, making all that quarter of the pond, perhaps half an acre, as smooth as a looking-glass.

After this I contrived to take with me, whenever I went into the country, a little oil in the upper hollow joint of my bamboo cane, with which I might repeat the experiment as opportunity should offer, and I found it constantly to succeed.

In these experiments, one circumstance struck me with particular surprise. This was the sudden, wide and forcible spreading of a drop of oil on the face of the water, which I do not know that anybody has hitherto considered.

If a drop of oil is put on a highly polished marble table, or on a looking glass that lies horizontally, the drop remains in its place, spreading very little. But, when put on the water, it spreads instantly many feet round, becoming so thin as to produce the prismatic colors, for a considerable space, and beyond them so much thinner as to be invisible, except in its effect of smoothing the waves at a much greater distance.

It seems as if a mutual repulsion between its particles took place as soon as it touched the water, and a repulsion so strong as to act on other bodies swimming on the surface, as straw, leaves, chips, etc., forcing them to recede every way from the drop, as from a centre, leaving a large, clear space.

The quantity of this force, and the distance to which it will operate, I have not yet ascertained; but I think it a curious inquiry, and I wish to understand whence it arises.

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Our friend, Sir John Pringle, being soon after in Scotland, learned there, that those employed in the herring fishery could at a distance see where the shoals of herrings were, by the smoothness of the water over them, which might possibly be occasioned, he thought, by some oiliness proceeding from their bodies. A gentleman from Rhode Island told me it had been remarked that the harbor of Newport was ever smooth while any whaling vessels were in it, which probably arose from hence, that the blubber which they sometimes bring loose in the hold, or the leakage of their barrels, might afford some oil, to mix with that water, which from time to time they pump out, to keep their vessel free, and that some oil might spread over the surface of the water in the harbor, and prevent the forming of any waves.

This prevention I would thus endeavor to explain:

There seems to be no natural repulsion between water and air, such as to keep them from coming into contact with each other. Hence we find a quantity of air in water, and if we extract it by means of the air pump, the same water, again exposed to the air, will soon imbibe an equal quantity.

Therefore, air in motion, which is wind, in passing over the smooth surface of water, may rub, as it were, upon that surface and raise it in wrinkles, which, if the wind continues, are the elements of future waves.

* * * * * * * * *

Thus, the small, first-raised waves, being continually acted upon by the wind, are, though the wind does not increase in strength, continually increased in magnitude, rising higher and extending their bases, so as to include a vast mass of water in each wave, which, in its motion, acts with great violence.

But if there be a mutual repulsion between the particles of oil, and no attraction between oil and water, oil dropped on water will not be held together by adhesion to the spot whereon it falls; it will not be imbibed by the water; it will be at liberty to expand itself; and it will spread on a surface that, besides being smooth to the most perfect degree of polish, prevents, perhaps by repelling the oil, all immediate contact, keeping it at a minute distance from itself; and the expansion will continue till the mutual repulsion between the particles of the oil is weakened and reduced to nothing by their distance.

Now, I imagine that the wind, blowing over water thus covered by a film of oil, cannot easily *catch* upon it, so as to raise the first wrinkles, but slides over it, and leaves it as smooth as it finds it. It moves a little the oil indeed, which, being between it and the water, serves it to slide with, and prevents friction, as oil does between those parts of a machine that would otherwise rub hard together.

Hence, the oil dropped on the windward side of a pond proceeds gradually to leeward, as may be seen by the smoothness it carries with it, quite to the opposite side. For the wind being thus prevented from raising the first wrinkles—that I call the elements of a wave—cannot produce waves, which are to be made by continually acting upon and enlarging those elements, and thus the whole pond is calmed.

Totally, therefore, we might suppress the waves in any required place, if we could come at the windward place where they take their rise. This, in the ocean, can seldom if ever be done. But perhaps something may be done on particular occasions to moderate the violence of the waves when we are in the midst of them, and prevent their breaking, where that would be inconvenient.

For, when the wind blows fresh, there are continually rising on the back of every great wave a number of small ones, which roughen its surface and give the wind hold, as it were, to push it with greater force. This hold is diminished by preventing the generation of those small ones. And possibly, too, when a wave's surface is oiled, the wind in passing over it may rather in some degree press it down, and contribute to prevent its rising again, instead of promoting it. This, as mere conjecture, would have little weight if the apparent effects of pouring oil into the midst of waves were not considerable, and as yet not otherwise accounted for.

When the wind blows so fresh as that, the waves are not sufficiently quick in obeying its impulse; their tops being thinner and lighter, are pushed forward, broken, and turned over in a white foam. Common waves lift a vessel without entering it; but these, when large, sometimes break above and pour over it, doing great damage.

But that this effect might in any degree be prevented, or the height and

violence of waves in the sea moderated, we had no certain account, Pliny's authority for the practice of seamen in his time being slighted.

Discoursing lately on this subject with his excellency, Count Bentinck, of Holland, his son, the Honorable Capt. Bentinck, and the learned professor, Allemand, a letter was mentioned, which had been received by the Count from Batavia, relative to the saving of a Dutch ship, in a storm, by pouring oil into the sea. I much desired to see that letter, and a copy of it was promised me, which I afterward received.

Extract of the letter from Mr. Tengnagel to Count Bentnick, dated Batavia, 5 January, 1770.

Near the islands Paul and Amsterdam, we met with a storm, which had nothing particular in it worthy of being communicated to you, except that the captain found himself obliged for greater safety in wearing the ship, to pour oil into the sea, to prevent the waves breaking over her, which had an excellent effect, and succeeded in preserving us. As he poured out but a little at a time, the East India Company owes perhaps its ship to only six demi-ames of olive oil.

I was present upon deck when this was done; and I should not have mentioned this circumstance to you, but that we have found people here so prejudiced against the experiment, as to make it necessary for the officers on board and myself to give a certificate of the truth on this head, of which we made no difficulty. * * *

Coming down to a later period, Capt. Anthony Jerome, ship *Black Warrior*, whaler, in the Arctic seas, makes the following statement:

The oil escaping through the lance wounds of a whale, always makes a slick to leeward, and it is well known to whalers that the pumping of bilge water from their ships, which is always more or less impregnated with oil will produce a slick.

In the year 1852, at the entrance of Behring's Straits, experienced a fearful gale and sea, soon after fastening to two sperm whales. Secured the ship to them with 600 feet of line, and rode out the gale with dry decks, in the smooth caused by the oil from the whales.

At another time was caught out in a heavy gale in a whale boat. Could not return to the ship. Made fast to a dead whale, with 100 feet of line, and laid in slick all night perfectly dry.

Capt. John Ward, ship *Electra*, off Tristan d'Acunha, South Atlantic, put long line to dead sperm whale, and rode out a fearful gale, with perfectly dry decks. Again, when on ship *General Williams*, he rode out a gale of four days' duration, while attached by long line to a large right whale, no water coming on deck during the whole gale.

The smooth sea, or slick, always to leeward of a dead whale is vouched for by numerous captains, and many cases can be cited of whalemen who have taken advantage of the smooth sea thus formed and have been saved much discomfort, alarm and danger.

The array of facts establishing the efficacy of oil in smoothing rough seas is so numerous, that the question naturally arises; Why has its general use been so long delayed?

I think there are several reasons.

Until recently, there were many who had never heard of the use of oil for quieting waves, though their whole lives had been spent at sea. Some were skeptical and indifferent, and others supposed that in order to derive any beneficial result, the oil must be used in large quantities. A few, perhaps, would have used it, had they been provided with proper appliances for its distribution. Very few ships have been in the habit of carrying as stores, a sufficient quantity of oil to spare for wave oiling, not even the small amount necessary to use through one storm. No doubt, many shipwrecks have occurred, which might have been prevented, had the oil at hand been used, instead of letting it remain securely sealed up.

The Hydrographic Office deserves much credit for the interest created in the use of oil, and the information it has given to mariners regarding its application and effectiveness, by collecting and publishing the experiences of those who have experimented with it. A portion of its monthly *Pilot Chart* has been devoted to these reports, thus keeping the subject constantly before those most interested. These reports have since been published in book form by the Hydrographic Office for general distribution.

From pamphlet No. 83, entitled "The Use of Oil to Lessen the Dangerous Effect of Heavy Seas," I quote the following:

> Experience seems to demonstrate that the thick and heavy oils are generally the best for this purpose. Mineral oils are not so effective as vegetable or animal, and, therefore, the use of the first-named is not recommended when either of the latter is available. It may be remarked in this connection that crude petroleum probably gives good results in smoothing heavy seas, but its usefulness in this direction decreases in proportion to the degree to which it has been refined.
>
> It would be well to remember that soft oils, such as fish oils, cocoanut oil, and others of a like nature, become thick and useless when exposed to a very cold temperature, and if it becomes necessary to use them under this condition, it is advisable that they be mixed with some mineral oil, which has a much lower cold-test.

A comparatively small amount of oil, say two quarts per hour, properly used, is sufficient to prevent great damage both to vessels and small boats in heavy seas.

The greatest effect from oil is obtained when in deep water. In a surf, or where water is breaking on a bar, the effect of the oil is not so certain, but even in this case it may be of benefit, and its use is recommended.

In order to get the best possible effect from oil, it must be applied in such a way as to spread to windward.

It is effective when scudding, when lying-to, when wearing, and when lowering and hoisting boats in a heavy sea.

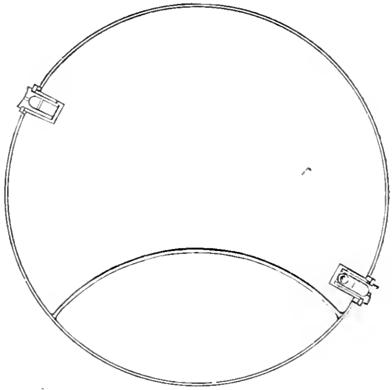
The best results seem to be secured by pouring it into the bowls of water-closets in which oakum has been placed, whence it slowly leaks out; and by means of canvas bags, having a capacity from one to two gallons. Oakum is stuffed in these bags, and they are punctured with a coarse sail-needle to facilitate the escape of the oil. In running before the wind, these bags should be suspended by lanyards from each cat-head and allowed to drag in the water. In lying-to, the weather-bow and mizzen-chains seem to be the best places for the bags, with sufficiently long lines to allow them to tend to windward while the ship drifts. In crossing a bar on a flood tide some oil should be put overboard and allowed to float in ahead of the boat, which should follow with an oil-bag towing astern. In crossing a bar against an ebb tide, no advantage can be obtained by using oil from the boat. For boarding a wreck, a vessel should run as close as possible under the lee of the wreck and put the oil over. The wreck will soon drift down into the oil, when a boat can be sent alongside of her most favorably. In the case of a boat riding to a sea-anchor in heavy weather, the oil-bag should be secured to an endless line rove through a block at the sea-anchor, by means of which the oil is spread well ahead of the boat, and when the bag is empty, it can be hauled on board and replenished. A similar system could be employed to advantage by the fishermen on the banks.

Those who have made practical tests seem to be unanimous in their belief in the efficacy and practical utility of oil for lessening the dangerous effect of heavy seas. They differ somewhat in opinion in regard to the minimum quantity required per hour.

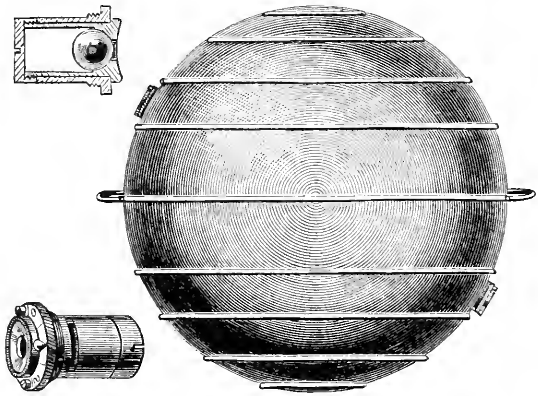
This amount cannot be definitely ascertained until some better mode of distribution is adopted. A perforated bag containing oakum or waste, saturated with oil, will always give an uncertain and irregular flow. Even if the necessary amount were known, it would be an impossibility to so perforate a bag that it would give forth a pre-determined quantity.

This uncertainty makes the bag objectionable, especially for use at night. Excessive quantities of oil are apt to be used in order to be on the safe side, and on this account, the use of the bag

ceases to be economical. After using, it is apt to be stowed away out of sight, in some locker, to save for future use, and it then becomes quite as dangerous as heavy seas, by the possible and probable danger of its becoming a fire-brand from spontaneous combustion.



In view of these facts and the many inquiries for some better mode for distributing the oil, I have devised an oiler, which, I believe, to be philosophic in principle, simple in construction and adapted for the purpose in question. My device (*see engravings*) consists



of a hollow metal globe, ten inches in diameter, and about one and one-half gallons capacity. To protect it from injury and to add to its strength, heavy wire rings are soldered around the outside.

It has an air chamber to float it in an upright position, and an upper and lower valve to regulate the flow of the contents. By

means of a ball in the upper valve, the flow of oil is stopped automatically, when action is not desired.

When filled with oil, the upper valve is adjusted to give vent to the oil in any desired quantity, and the lower valve is set to admit the water. When placed in the sea, the water coming in at the lower valve, by reason of its greater specific gravity, steadily and regularly displaces the oil, which flows out through the upper graduated valve.

After the vessel has emptied itself of oil, it should be taken in and refilled, or replaced by another. When it is not practical or desirable to put it in the sea, it can be placed in the bowl of the water closet, or used as a drip by securing it to any portion of the ship. The flow will be regulated by the valves.

A piece of tubing leading from the valve to the water may be used to prevent the oil from blowing to leeward.

The portability of this oiler adapts it for use in any part of the ship, or for small boats. As it is buoyant, it can be anchored to protect any place or wreck while landing passengers or crew.

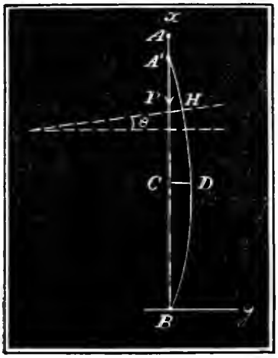
Its capacity for distribution ranges from one pint to two gallons per hour in a continuous and regular flow. When not in service, it can be put away and is always ready for immediate use.

Its small cost, its economical use of oil, its certainty of action, and its adaptation to all the conditions in which wave oiling can be made beneficial, make it a desirable distributor.

NOTE ON LONG COLUMNS.

By WM. CAIN, Prof. Math. and Eng., S. C. Mil. Academy.

In none of the various discussions of long columns that have appeared from time to time in the American journals, have I seen a reference to a formula given by Bresse (in his *Mécanique Appliquée*, premiére partie, p. 372,) who takes for the reciprocal of the radius of curvature, the correct expression $\frac{d\,\theta}{d\,s}$ in place of the usual approximation $\frac{d^2\,y}{d\,x^2}$ and thus deduces ultimately a more satisfactory form than the well known Euler's formula.



It may be of interest, therefore, to give a brief statement of Bresse's analysis with comments; likewise to deduce a formula that involves a criticism of the Rankine formula, with a mention of other formulæ that have been recently proposed as representing the results of experiments. Thus, let the axis of the column $A\ B$ in the figure, *hinged at the ends*, take the position $A'\ B$ under the action of the force P directed from A to B. Now, in the beginning, we must note a radical defect in all column formulæ in this; that if the column is prismatic and homogeneous and P acts along the axis, supposed originally straight, the column should decrease in length somewhat, but *remain straight* after P is applied, since the compression is uniform throughout. There is thus no possibility

for the column to deflect, unless the force P is eccentrically applied, or the column is either not straight or not homogeneous.

Practically, the column deflects originally from some of these causes, but as they are accidental, they are not included in the analysis, which simply expresses the conditions of equilibrium between the force P and the molecular actions developed in any section whatsoever of the column, as at H, after the column is in some *accidental way*, not included in the formula sprung out of line, and afterward the accidental cause is removed.

It can hardly be expected that a formula that leaves out an important part of the actual conditions of the case, should give absolutely correct results; it is, moreover, incorrect to expect the formula, deduced on the supposition of perfect elasticity, to give results agreeing with experiments on the so-called *breaking strength* of columns, if the limit of elasticity has been much exceeded, so that Hooke's law no longer holds, even approximately.

Bearing in mind then, that the formula that will be deduced, does not account for *how* the column became bent in the first instance, we shall proceed to express the conditions of equilibrium necessary to keep it bent after the first cause has been removed. Thus, we shall call $d\,s$ the primitive length of an element of the axis at H, and θ the inclination of the section at this point to its original horizontal direction. The section infinitely near this, which was originally at the distance $d\,s$, makes now the angle $d\,\theta$ with the other section.

Hence, calling y the horizontal ordinate to the axis of the curve, we have the conditions of equilibrium between the molecular stresses and the force P, given by the known equation of moments,

$$e\,r^2 \frac{d\,\theta}{d\,s} = P\,y \tag{1}$$

where $e = E\,Q$ or the product of the modulus of elasticity by the area of the cross section and r represents the radius of gyration.

The element whose original length was $d\,s$, has now, in consequence of the uniform compression P, a new length $d\,s'$; whence

$$d\,s' = d\,s\left(1 - \frac{P}{e}\right) \tag{2}$$

on neglecting the small inclination of P to the normal at the section.

Finally, since the component of P parallel to the section is also very small, it is permitted to neglect the influence of transverse shearing, and to regard a section originally normal to the axis as likewise normal after deformation. Hence, regarding x and y as the co-ordinates of the final elastic curve,

$$- d\, s' \sin \theta = d\, y \tag{3}$$

From equations (2) and (3), we find

$$\frac{d\, y}{d\, s} = - \sin \theta \left(1 - \frac{P}{e}\right),$$

which substituted in (1), after having differentiated it in relation to s, gives

$$e\, r^2 \frac{d^2 \theta}{d\, s^2} = - P \left(1 - \frac{P}{e}\right) \sin \theta\,;$$

whence, multiplying by $2\, d\, \theta$ and integrating, we have

$$e\, r^2 \left(\frac{d\, \theta}{d\, s}\right)^2 = 2\, P \left(1 - \frac{P}{e}\right) \cos \theta + C$$

If we call θ_0 the value of θ at A', and make $y = o$ when $\dfrac{d\, \theta}{d\, s} = o$ from (1), we can determine C in terms of θ_0:

$$\therefore e\, r^2 \left(\frac{d\, \theta}{d\, s}\right)^2 = 2\, P \left(1 - \frac{P}{e}\right)(\cos \theta - \cos \theta_0) \tag{4}$$

or

$$\frac{d\, \theta}{\sqrt{2\,(\cos \theta - \cos \theta_0)}} = \frac{d\, s}{r} \sqrt{\frac{P}{e}\left(1 - \frac{P}{e}\right)}$$

The integral of the second number of this equation, between the limits $s = o$ and $s = A\, B = 2\, a$ is $\dfrac{2a}{r} \sqrt{\dfrac{P}{e}\left(1 - \dfrac{P}{e}\right)}$; the integral of the first between the same limits is known from the theory of the simple pendulum. If θ becomes zero i times between B and A', this integral will be the time of i oscillations of a simple pendulum, having for a length the number g, or the acceleration due to gravity; we should have then approximately for its value $i\, \pi \left(1 + \dfrac{\theta_0^{\,2}}{16}\right)$, neglecting in the differential expression powers of θ_0

superior to the fourth;* consequently we have

$$i\pi\left(1+\frac{\theta_0^2}{16}\right)=\frac{2a}{r}\sqrt{\frac{P}{e}\left(1-\frac{P}{e}\right)}. \tag{5}$$

Now equations (1) and (4) applied to points, such as D, for which θ becomes zero at the same time that y takes a maximum value f, give

$$e\,r^2\frac{d\,\theta}{d\,s}=P\,f,$$

$$e\,r^2\left(\frac{d\,\theta}{d\,s}\right)^2=2\,P\left(1-\frac{P}{e}\right)(1-\cos\theta_0),$$

from which we draw

$$\frac{P\,f^2}{e\,r^2}=2\left(1-\frac{P}{e}\right)(1\cos\theta_0)$$

or in consequence of the smallness of θ_0, since

$$2\,(1-\cos\theta_0)=2\,(1-1+\tfrac{1}{2}\,\theta_0^2-\ldots)=\theta_0^2,$$

nearly,

$$\frac{P\,f^2}{e\,r^2}=\left(1-\frac{P}{e}\right)\theta_0^2 \tag{6}$$

Eliminating θ_0 between (5) and (6), we find

$$\frac{P\,f^2}{e\,r^2\left(1-\frac{P}{e}\right)}=16\left[\frac{2\,a}{i\,\pi\,r}\sqrt{\frac{P}{e}\left(1-\frac{P}{e}\right)}-1\right] \tag{7}$$

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* From the formula,

$$\cos x=1-\frac{x^2}{2}+\frac{x^4}{2.3.4}-\ldots$$

we have,

$$2\,(\cos\theta-\cos\theta_0)=\theta_0^2-\theta^2-\frac{1}{12}\left(\theta_0^4-\theta^4\right)$$

$$=\left(\theta-\theta^2\right)\left[1-\frac{1}{12}\left(\theta_0^2+\theta^2\right)\right];$$

neglecting powers of θ higher than the fourth.

Whence

$$2\,i\int_0^{\theta_0}\frac{d\,\theta}{\sqrt{2\,(\cos\theta-\cos\theta_0)}}=2\,i\int_0^{\theta_0}\frac{d\,\theta}{\sqrt{\theta_0^2-\theta^2}}\left[1-\frac{1}{12}\left(\theta_0^2+\theta^2\right)\right]^{-\frac{1}{2}}$$

$$=2\,i\int_0^{\theta_0}\left(1+\frac{1}{24}\theta_0^2+\frac{1}{24}\,\theta^2\right)\frac{d\,\theta}{\sqrt{\theta_0^2-\theta^2}}=i\,\pi\left(1+\frac{\theta_0^2}{16}\right).$$

a formula from which the greatest deflection f can be found for an assumed load P, and vice-versa. There is evidently no deflection when the [ ] in the right number is negative; so that by placing it equal to zero, we can find the lower limit of P, at which the previous theory begins to be applicable. For slightly greater values of P, f becomes appreciable and the bending is permanent, since the *moment* $(P\,y)$ at any cross section by equation (1) exactly balances the *moment* of *all* the normal molecular stresses at the cross section. We say *all* advisedly since the stresses at the section due to flexure alone, added to those caused by the uniform compression P, together form the real stresses there whose *moment*, however, about an axis of flexure passing through the axis of the beam, is equal to the moment of the stresses that would be caused by flexure alone. We see, therefore, that the uniform stress is not neglected in the investigation; but that equation (7) represents the conditions of equilibrium between the force P and the molecular stresses when, *after the beam has been bent from some accidental cause, the cause is removed, and the force P is sufficient to keep the beam bent to an extent given by the formula.* Increasing the load, increases the deflection f, so that we can compute the value of P corresponding to any value of f assumed as safe.

The formula does not apply to *safe loads*, for such values of P will always be found to be too small to keep the column bent, even if it should be sprung out of line in some accidental way, so that the formula is no longer applicable, and direct uniform compression is alone to be provided for at each cross section; so that in any practical case the theory of bending does not come in at all, though possibly a little deflection is in practice always experienced from eccentricity of the load, crookedness and want of homogeneity of the column, none of which items can be included in the formula, as they vary for each separate column and cannot be known before-hand.*

This strictly exact theoretical formula (7) enables us to ascertain the minimum theoretical load at which bending can be just main-

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* Hodginson says in this connection: "I have many times sought experimentally, with great care, for the weight producing incipient flexure (in columns) according to the theory of Euler, but have hitherto been unsuccessful. So far as I can see, flexure commences with weights far below those with which pillars are usually loaded in practice.

tained, which can be found by putting the quantity in [ ] equal to zero, and solving for P. If we neglect $\frac{P}{e} = \frac{P}{E\,Q}$ as very small in comparison with 1 (since it represents the shortening of the column per unit of length), we derive, in the way mentioned, the well known Euler's formula,

$$P = \left(\frac{i\,\pi}{2\,a}\right)^2 e\,r^2 = \left(\frac{i\,\pi}{2\,a}\right)^2 E\,I \tag{8}$$

giving the least value to P, at which bending can just begin to be maintained for i repetitions of the curvature shown by the figure. The least force is $P = \left(\frac{\pi}{2\,a}\right)^2 E\,I$, corresponding to $i = 1$. By making $i = 2, 3, \ldots$, we have values of P, equal to the first multiplied by $4, 9, \ldots$; so that the column cannot remain bent if it does not slightly exceed the lower limit $\frac{\pi^2}{4\,a^2}\,E\,I$. But since equilibrium can possibly subsist for $4, 9, \ldots$ times the value, if the curvature is compounded, we have another reason for not expecting the results of all experiments to agree with one formula (8), where i is only given one of its possible values, as is usual.

If desired, we can write a slightly more accurate formula for P by not confounding the final and primitive lengths of the column, as was done in deducing (8). Thus where bending is just about to begin the [ ] in (7) is zero, which gives for the least value of $\frac{P}{e}$ corresponding,

$$\frac{P}{e} = \frac{1}{2}\left(1 - \sqrt{1 - \frac{i^2\,\pi^2\,r^2}{a^2}}\right)$$

or expanding, we have approximately

$$\frac{P}{Q} = \frac{i^2\,\pi^2\,r^2}{l^2}\left(1 + \frac{i^2\,\pi^2\,r^2}{l^2}\right) E \tag{8a}$$

after placing $l = 2\,a =$ length of column. On neglecting the second term in the parenthesis, as generally small compared with 1, we are conducted at once to formula (8).

Referring again to formula (7), and observing that $\frac{P}{e} = \frac{P}{E\,Q}$ is always very small compared with 1, we make no appreciable

error in neglecting it in the terms $\left(1 - \frac{P}{e}\right)$, giving us for the square of the deflection at the middle of the column,

$$f^2 = \frac{16\, e\, r^2}{P}\left[\frac{2\,a}{i\,\pi\,r}\sqrt{\frac{P}{e}} - 1\right];$$

or, observing that $e = E\,Q$, and therefore $e\,r^2 = E\,Q\,r^2 = E\,I$, where I is the moment of inertia, about the neutral axis passing through the centre of gravity, of the cross section, we deduce

$$f^2 = 16\left(\frac{2\,a}{i\,\pi}\sqrt{\frac{E\,I}{P}} - \frac{E\,I}{P}\right) \tag{9}$$

Now for values of P, greater than given by Euler's formula (8), which makes the term in the parenthesis in (9) zero, we note that as P increases, the parenthesis increases, since $\sqrt{\frac{E\,I}{P}}$ diminishes less rapidly than the term $\frac{E\,I}{P}$ for the same increase in P; therefore we conclude, as has been before stated, that f, *the deflection at the centre, increases with P for values above those given by* (8). This increase in f is so rapid for a small increase in P over that given by (8), that the breaking unit stress is soon exceeded as deflection proceeds; so that if perfect elasticity was maintained up to the bending limit, we should expect Euler's formula (8) to give us very nearly the breaking weight, especially in the case of very long columns, where the unknown eccentricity in the position of P (especially in the case of columns with fixed ends) has not comparatively so great an influence as in the case of short columns; besides the theory is more nearly realized in the case of very long columns, where the bending can be quite appreciable before rupture begins or even the limit of elasticity is exceeded, though in the latter case, as we have seen, it requires but a small additional load to increase f considerably, and thus cause rupture with a load only slightly exceeding or approximately equal to that given by Euler's formula. This theoretical deduction is sustained by experiment as we shall see later, and it is plain that experiment alone can give us the limit of error made in any case. It is evident likewise that for columns of the ratio of length to diameter generally used, that the eccentricity in the load, etc., will generally

cause the crippling weight, as determined by experiment to be less than that deduced by aid of Euler's formula, which is found to be the case.

Euler's formula is not generally demonstrated in a very satisfactory manner. Thus, starting with the well-known equation,

$$E\,I\frac{d^2 y}{d\,x^2} = -\,P y$$

we easily deduce, in the usual manner,

$$y = f\ sin\left(x\sqrt{\frac{P}{E I}}\right)$$

But since y must be zero for $x = 2\,a$, it follows that $2\,a\sqrt{\frac{P}{E\,I}}$ must be a multiple of the semi-circumference, so that if i is a whole number

$$2\,a\sqrt{\frac{P}{E\,I}} = i\,\pi \;\therefore\; P = \frac{i^2\,\pi^2}{4\,a^2}\,E\,I.$$

Now it is a singular fact that the above value of y is not satisfied by any other values of P than those given by the last equation, unless f is zero, since no other value will give $y = o$ for $x = 2\,a$; but for such values of P, we know from equation (9) above that f is zero, or that the so-called elastic curve reduces to a straight line; but the ordinary analysis leaves this important fact undetermined. In fact, it would seem from it that any value of P greater than the value given by the last equation was impossible, and would immediately cause rupture, since the value, $y = f\,sin\,x\sqrt{\frac{P}{E\,I}}$ is no longer satisfied, for $y = o$, $x = 2\,a$, since $2\,a\sqrt{\frac{P}{E\,I}}$ is, by assumption, no longer a multiple of the semi-circumference; whereas, by equation (9) we can easily deduce the value of P corresponding to *any* assumed f.

By the ordinary analysis, therefore, we cannot assert, for values of P different from those given by Euler's formula, that the conditions of equilibrium are satisfied, or that the elastic curve is a *sinusoid;* in fact, for the one possible case, where P has just the value given by Euler's formula, the neutral line becomes a right line.

We see what an improvement Bresse's analysis is over the old, but even here the equation of the elastic curve is not found—only the expression for the middle ordinate is known.

The formulæ for columns "fixed at one end only," and "fixed at both ends," are easily derived from the preceding.

Thus for columns *fixed at one end and free at the other*, the elastic curve is the same as the part $A'\,D$ in the figure, and the deflection $C\,D$ is the same as in the first case.

If we make $i = 1$, and call the length of the column a, formulæ (7), (8) and (9) apply directly for the one case of bending represented by $A'\,D$ in the figure.

Similarly for columns *fixed at both ends*, so that the end tangents to the centre line coincide with the original axis of the column, we must conceive the column represented in the figure to be extended above A' and below B a distance $= A'\,D = D\,B$, with the same curvature as the part $A'\,D$, only concave to the right.

The deflection of this column at the centre, D, from the line of the end tangents, which are now parallel to $A\,B$, is $2 \,.\, \overline{C\,D}$.

Hence if we call $2\,a$ the length and f' the centre deflection of the column fixed at the ends, we have only in (7), (8) and (9) to put (a) for $(2\,a)$ and $\frac{1}{2}f'$ for f to get the corresponding formulæ for the case of the column fixed at the ends. Therefore, we have for this case the deflection at the centre, when there are no repetitions of the simplest kind of elastic curve that can be supposed, from (7),

$$\frac{P\,f'^2}{e\,r^2\left(1-\frac{P}{e}\right)} \doteq 64\left[\frac{a}{\pi\,r}\sqrt{\frac{P}{e}\left(1-\frac{P}{e}\right)}-1\right];$$

or very nearly from (9),

$$f'^2 = 64\left(\frac{a}{\pi\,r}\sqrt{\frac{E\,I}{P}}-\frac{E\,I}{P}\right);$$

and finally, in order that any bending be possible we must have

$$P > \left(\frac{\pi}{a}\right)^2 E\,I.$$

The condition that there shall be bending in the three cases, can then be expressed generally as follows:

$$P > \left(\frac{\pi}{c}\right)^2 E\,I;$$

where, if l is the length of column in each case, $c = 2\,l$ for column fixed at one end only; $c = l$ for column hinged or round at both ends, and $c = \frac{l}{2}$ for column fixed at both ends.

In the experimental formulæ of Mr. Johnson, given further on, Euler's formula for round ends is adopted, for that case, and the value of c in terms of l is determined by the experiments for the other cases.

Various attempts have been made, from time to time, to replace Euler's formulæ by other *rational* formulæ, but so far without success.

(*To be continued.*)

On RANSOME'S IMPROVEMENTS in the MANUFACTURE of PORTLAND CEMENT.*

By R. J. Friswell, F,I.C., F.C.S.

(*Concluded from vol. cxxiii, page 475.*)

The next question to be considered is the economy of fuel effected by the use of gas producers. Instead of consuming coke, these require only to be fed with slack, coal dust, or anything that will burn, fed in at intervals through a hopper. A two-cylinder works would require for its daily service probably one gas producer, capable of converting about six hundredweights of slack per hour into gas. In addition, there should be one similar producer in reserve kept going at only one-fifth of its full power, its gas being utilized under the boiler or drying floors; it would thus always be ready in case of a breakdown to take up its full production and supply the revolvers in motion. These producers are chambers of brick-work, in which a portion of the fuel burning gasifies the rest, a small jet of steam being blown in to assist the operation. They consume the whole of the coal, nothing escaping but ashes, and thus alone effect a great saving in the stoppage of the waste of cinders inevitable under ordinary circumstances. Numerous first-rate makers exist, and the use of gas producers is daily extending in the country, even in places where coal is raised either on or in close proximity to the works. Their value as economizers is recognized in all furnace operations requiring intense local heats. In steel and glass-melting industries employing heats like that of the cement kiln, it is asserted that, coupled with the use of regenerators, a saving of fifty to seventy per cent. of the fuel formerly used is effected. The only use for which their value is disputed is for steam raising, though in works where they are

* Reprinted from *The Engineer*, London, March 3, 1887.

required for other purposes, even here they would effect savings in fire-bars and in wages. Their cost is small, they occupy little room, they can be placed at any reasonable distance from the place where the gas is to be burnt, so as to be in close proximity to the coal siding, any laborer can shovel the slack into them, and they do not require constant skilled supervison. As we have before stated, there are several forms in use, that of Wilson being one of the best.

On the occasion of my inspection of Mr. Ransome's experimental furnace at Grays, Essex, the revolver was furnished with gas from a small producer, built by the works' bricklayer, which was gasifying about two hundredweights slack per hour; this not only supplied the furnace, but the valve was partly shut down to control the gas, which was, nevertheless, in excess of what was required. In fact, I felt convinced that the producer could have supplied two half-ton revolvers. There was accordingly here exhibited a consumption of about two hundredweights slack per ton of cement produced, instead of the usual seven to ten hundredweights coke per ton of cement clinker from the kiln. The results derived from this plan of gas firing are, therefore: (1.) Possibility of working with regenerative furnaces, thus saving all heat passing from the revolver. (2.) Use of about three hundredweights cheap slack per ton of cement instead of seven hundredweights coke. (3.) Complete combustion of all fuel, the steam injected being decomposed by the red-hot cinders, and producing carbonic oxide and hydrogen. In all ordinary furnaces great quantities of fuel are lost, in the shape of cinders inextricably mixed with the ash or mineral matter of the coal. (4.) The cement is kept entirely free from fuel ash.

In addition to these, the revolver gives us the following advantages: (1.) Economy of space, two revolvers with their appurtenances, and one in reserve, covering 900 square feet, turning out the same weight of cement as eleven kilns covering 4,400 feet. (2.) Continuous day and night working, and hence economy of fuel lost by necessary cooling, and subsequent re-heating of the kiln walls. (3.) Economy of repairs, which are simple and cheap. (4.) Less frequent need of repairs, as the continuous heat involves no racking like the alternate heating and cooling. (5.) Economy in first cost. (6.) Economy in grinding, a granular sand being pro-

duced instead of lumps of clinker, whereby crushers are quite abolished, and the wear and tear of the mill-stones greatly reduced. (7.) Economy of hand labor. Revolver cement can be handled on the American elevator system. (8.) Improved quality from (*a*) non-mixture with fuel ash; (*b*) no over-burning nor under-burning. (9.) Increased control over quality of cement, it being possible to stop, increase or diminish the flow of crushed slurry and to vary its quality at any time. (10.) Freedom from loss by accident. The ordinary kiln once charged and fired must burn out, whether charged wrongly or rightly, while, as before stated, any error in material can be rectified in a revolver as soon as discovered. (11.) Perfect control of temperature. And lastly (12) power of varying temperature according to nature of material. On the other side, there are the inherent defects of the kiln process, which need not here be recapitulated, having been fully treated already, and, moreover, being indicated by contrast in the above summary. It will have been noticed in the description that the time occupied by the crushed slurry in passing through the cylinder is about half an hour. Compared with the time occupied by the kiln process, this will seem to the practical cement-maker a very short—perhaps too short—a time to effect the necessary changes. It must, however, be remembered that a very great part of the kiln operation is taken up in warming the large lumps of dried slurry, which, like all earths, are very bad conductors of heat. This is not needed in the revolver, as the coarse powder almost directly attains the heat of the cylinder. The small size of the particles also permits the rapid liberation of the carbonic acid from the chalk, and after that is done it is not necessary to keep them heated, the necessary combination of the lime and clay taking place as soon as the particles are sufficiently hot, and this occurs long before the end of the cylinder is reached.

It must be remembered that a great part of the time taken in the kiln is necessary, by reason of its construction and the great mass of matter it contains, to the upper portions of which the fire can only reach after the lower parts have parted with most of their carbonic acid and moisture, which as they pass upward prevent the layers of coke above from burning. Much of the moisture also condenses in the upper part, only to be again driven out when the fire reaches it; this does not take place in the revolver, the

gases rushing quickly past to the regenerator and meeting only a moderate quantity of cold powder on its way in. It is also almost needless again to point out that at least one-seventh of the time is taken up in loading, another seventh in cooling, and a third, together nearly half, in unloading. Of the remainder it is to be observed that most of it is used in the gradual advance of the fire upwards, so that it may be safely stated that the lower layers of the kiln are sufficiently burnt a few hours after the start, and from then to the complete burning of the upper part they are simply lying idle. Could they be removed, as is the case in the revolver, they might at once be ground; but this removal is, from the nature of the method, impossible.

In addition to the method of burning just described, Mr. Ransome has introduced another improvement, which, however, is available only in certain districts. This is the introduction of a new material in cement-making in the form of blast-furnace slag. This is, as is well known, produced in enormous quantities, and notwithstanding innumerable attempts to utilize it, it is in truth a valueless waste material, costing the ironmaster large sums either for carriage to sea or for land to deposit it on. The ores used mostly for the production of iron consist of the carbonate of that metal mixed with silica and alumina. The latter are removed by the addition of lime, in the shape of limestone, to the furnace charge. The lime combines with the silica and alumina to form a fusible substance. This continually runs from the furnace and constitutes the slag in question. It contains the same elements as Portland cement, though in different proportions, as the following analyses will show:

	Middlesborough Slags.			*Portland Cements.*		
Ferrous oxide,	0·72	3·64	0·61	3·41	5·46	Ferric oxide.
Manganous oxide,	0·35	1·02	trace	. .	. .	
Alumina,	24·69	20·70	22·28	6·92	8·00	Alumina.
Lime,	40·00	30·88	40·45	50·00	55·57	Lime.
Magnesia,	3·55	4·25	7·21	0·82	0·77	Magnesia.
Silica,	27·65	30·40	27·80	24·07	22·92	Silica.
Potash,	0·46	0·50	0·90	0·73	1·13	Potash.
Soda,	0·90	0·00	0·00	0·87	1·70	Soda.
Sulphur,	1·95	1·34	2·00	0·67	1·70	Sulphur.
Phosphorus,	9·25	0·00	0·00	. .	. .	

Neglecting the non-essential constituents, we have then a body containing silica in nearly the same proportion as does cement, lime in nearly two-thirds the proportion, and alumina in nearly a three-fold proportion.

If it be admitted that the functions of the alumina and silica in cement are reciprocal,* or nearly so, it follows that we may add these substances together, when we shall have the following results, again, for the sake of clearness, omitting the non-essential substances:

	Middlesborough Slag			*Portland Cements.*	
	1	2	3	1	2
Alumina and silica,	52·34	51·12	50·08	30·80	30·92
Lime,	40·00	36·88	40·45	59·90	55·67

From this it will be seen that in the slag the ratio of lime to the two other bodies is about 39 : 51, while in the cement it is 58 : 31. A simple calculation will show that on this basis the $39 + 51 = 90$, or including the other bodies, 100 parts of slag requires the addition of 56 parts of lime = 100 parts of calcium carbonate, *i. e.*, dry chalk or limestone, to give a substance yielding a good cement. The only difficulty is the hardness of the slag, but this is overcome by Mr. C. Wood's method of running it into water, when it disintegrates, and yields a slag sand, which is easily ground with the chalk after separation of a little entangled

* On this point much has been written and many conflicting theories suggested (*Chem. News*, 1865, p. 152, etc.). Fremey has perhaps done the most practical work on the subject, when, as long ago as 1865, he prepared pure silicates and aluminates of calcium, and showed that the former alone had no hydraulicity, while the latter had that property when the ratio of alumina to lime lay between one and three molecules of lime to one of alumina; but he divided the setting process into two parts, in which he believed that both aluminates and silicates took part, the former by hydration, the latter by combination with hydrate of lime. Some years later (*Dingler's Polyt. Jour.*, vol. ccxv, pp. 538–552, and ccxvi, pp. 63–77) Erdmenger published a long and elaborate paper on the cause of hydraulicity in Portland cement. His results agreeing substantially with those arrived at by Fremey, led him to point out that the nearer a cement approaches to a two-fifth silicate the better. Two cements of this character he mentions as equally good. They had the following composition:

	Silica.	*Alumina and Oxide of Iron.*	*Lime.*
No. 1,	24·0	9·0	65·5
No. 2,	20·7	14·4	64·0

Thus showing considerable latitude in the relative proportions of alumina and silica. The three Middlesborough slags, if treated as suggested, would yield cements approximating to the following proportions:

	Silica.	*Alumina.*	*Lime.*
No. 1,	18	15	62
No. 2.	19	13	60
No. 3,	18	14	62

iron by sifting. Attempts have long since been made to produce this cement in the ordinary kiln, but have been abandoned, as the mixture has no coherence, and the lumps fell to pieces as soon as they got hot, and choked up the draught, putting the fire out.

This friability, disastrous in the kiln, is no defect but rather virtue in the revolver, so that in this matter the two inventions supplement each other, and the revolver thus brings a new cement material to the fore. It must, of course, be understood that slag cement cannot be made to compete with chalk-clay cement except in the neighborhood of iron works. In the South of England, the cost of carriage of the slag prevents it from being used, as clay is to be had on the spot. There is, however, every reason to believe that the slag cement may prove a profitable manufacture in the iron districts where the cement revolvers could themselves be fired by means of the blast furnace gases which are now everywhere being utilized as fuel.

Careful tests of the new material have been made, from which it would appear that slag cement attains its strength more rapidly than does ordinary Portland, for it was found that Ransome's had a breaking strain of 1,440 pounds, on an area of two and one-fourth square inches in twenty-eight days, while the Portland reached 1,325 pounds only in two years. The result is even more striking if short periods are taken, as the following shows:

Days.	*Portland 2¼ Square Inch Area, 123 lbs. per Bushel.* lb.	*Ransome's Slag Cement 2¼ Square Inch Area, 129 lbs. per Bushel.* lb.
2,	510	740
3,	698	870
7,	818	1170

It is therefore evident that this process is well worth the attention of cement-makers in our iron districts, while the revolver process of burning must before long come into universal use.

NOTE ON THE DISCHARGE OF TURBINE WATER WHEELS.

By J. P. Frizell.

A note published by me in the Journal of the Franklin Institute for August, 1883, called attention to an error in the method of estimating centrifugal force used by writers on the turbine water wheel. My statement amounted to nothing more than this: The centrifugal force developed in a rotating body depends on the motion of the body itself, and not on the motion of that which sustains or environs it. Applied to the turbine, this simply asserts that the centrifugal force acting on the water, depends on the motion of the water and not directly on the motion of the wheel.

Certain criticisms by Prof. Church, of Cornell, N. Y., (see Journal of the Franklin Institute, for May, 1884,) led me to publish in this Journal (July, 1884,) a method of computing the discharge of turbines, embodying my view of the action of centrifugal force. This method, applied to certain experiments made by James B. Francis in 1851, (see Lowell Hydraulic Experiments), gives, with remarkable exactness, the results of twenty-four different experiments, in which the velocity is practically the only variable element. The ordinary theory represents these experiments with no approach to correctness.

In the May number of this Journal (current year), Prof. Church assails the formula used in these computations in an effort to show that it has no rational foundation. His criticisms are entitled to an answer, because he is, in one point, correct.

My expression for the centrifugal force in that article, is

$$\textit{centrifugal force} = M\left((\omega - \omega_1)^2\, r - v\right), \tag{1}$$

in which M represents a mass of water so small that all its parts may be assumed to possess the same velocity. ω represents the angular velocity of the wheel, ω_1 that of the water with reference to the wheel, r the distance of M from the centre of rotation, v the radial velocity of the water.

To sum up the action of the centrifugal force upon the mass of water included between two floats of the wheel, I adopt a rough process of integration, founded upon measurements of the drawing

of the wheel and numerical computation, which avoids analytical difficulties, and leads to a substantially correct result.

Combining the expression so obtained with those deduced from other mechanical considerations, I find finally the following equation applicable to the wheel used in the experiments referred to.

$$1{\cdot}7277\, c^2 = 2\, g\, h + 5{\cdot}798\, \omega^2 - 1{\cdot}0844\, \omega\, c - 0{\cdot}5014\, c \qquad (2)$$

in which c is the velocity of discharge from the guide openings.

Prof. Church points out that these formulæ are not homogeneous, and give contradictory and inconsistent results under different suppositions. In this he is correct. Nevertheless, in affirming that they have no rational foundation, he disregards one of the cardinal maxims of philosophy; concluding too hastily and on too narrow a basis of fact. The formulæ contain one error, too trifling to have any practical bearing, yet none the less objectionable to a mathematician. Otherwise they are thoroughly well founded in mechanical principles, as I understand the latter, and are, in no sense, empirical.

On re-examining the matter, I find that the term v, in the expression for centrifugal force, was introduced upon erroneous considerations. The effect of the error was so slight that it did not disclose itself. I cheerfully discard the offending member. The expression for centrifugal force then becomes

$$\textit{centrifugal force} = M\, (\omega - \omega_1)^2\, r \qquad (3)$$

Remembering that $\omega - \omega^1$ is the angular velocity of the water, (3) is the ordinary expression for centrifugal force.

The correction, carried through to equation (2), simply cancels the last term in that equation and it becomes

$$1{\cdot}7277\, c^2 = 2\, g\, h + 5{\cdot}798\, \omega^2 - 1\,0844\, \omega\, c \qquad (4)$$

This equation is free from the difficulties found by Prof. Church. The application of this formula to the experiments in question is exhibited in the following table:

Number of the Experiment in Mr. Francis's Series.	n Number of Revolutions of the Wheel per Second.	$\omega = 2\pi n$ Angular Velocity of the Wheel. Feet per Second.	h Head acting on the Wheel. Feet.	Q Discharge in Cubic Feet per Second. By Experiment.	By Computation.
43	0·	0·	12·797	135·65	135·41
42	0·45431	2·8534	12·948	133·43	134·57
41	0·53232	3·3447	12·977	133·75	135·19
40	0·60000	3·7699	12·973	134·80	135·76
39	0·64702	4·0653	12·963	135·34	136·20
36	0·69471	4·3650	12·944	136·49	136·69
35	0·74211	4·6628	12·939	137·71	137·31
34	0·78401	4·9261	12·941	138·09	137·98
32	0·83624	5·2542	12·915	138·27	138·68
29	0·86643	5·4439	12·906	138·51	139·18
21	0·90201	5·6675	12·899	139·90	139·80
18	0·94507	5·9380	12·880	140·47	140·56
16	0·99945	6·2797	12·890	141·98	141·76
15	1·02373	6·4323	12·888	142·04	142·28
14	1·06744	6·7069	12·856	142·52	143·15
11	1·12518	7·0697	12·819	143·91	144·38
10	1·18460	7·4431	12·800	144·87	145·83
9	1·24514	7·8234	12·777	146·02	147·38
8	1·30933	8·2268	12·720	147·29	148·98
7	1·38249	8·6864	12·696	149·47	151·08
6	1·46149	9·1828	12·653	152·27	153·41
5	1·53218	9·6270	12·611	154·39	155·57
4	1·59651	10·0313	12·554	156·65	157·57
13	1·78404	11·2095	12·510	163·43	164·26

I have hoped for an opportunity of making a more extended application of this method, but the difficulty of obtaining the necessary data and the considerable labor involved, have thus far prevented. Only one occasion, in addition to the above, has presented itself for a test of the method. Mr. Hiram F. Mills, Engineer of the Essex Company, at Lawrence, Mass., has furnished me with a full size drawing of a Boyden turbine of eighty-four inches internal diameter, made by the Ames Company, of Chicopee, for the Washington Mills, at Lawrence, and tested as to discharge by Mr. Mills. The data for the complete verification of the computation cannot, of course, be exhibited as it would require the full size drawing to be published. Mr. Mills found the discharge under a head of twenty-nine feet and with a velocity of eighty revolutions per minute, 142 cubic feet per second. My method gives for the same conditions, 139·7 cubic feet per second.

Some other critical remarks of Prof. C. must be noticed. (1.) He says that I make the assumption "that if the sectional area of a guide passage (running full) is F and the '*velocity through F*' is $= c$, then the discharge through F is not $Q = F\,c$ but *five per cent. less.*" There is certainly a lack of conciseness in my article, on this point, but nothing of the contradiction he avers, and nothing that need occasion any difficulty to one familiar with the practical methods of hydraulics. c should have been defined not as the "velocity through F," but as the *velocity of discharge from F*, and similarly as to c_2. The velocity of discharge from an orifice is the velocity in the most contracted section, after passing the orifice. Practical hydraulicians would understand the matter correctly notwithstanding the definition, knowing that the velocity obtained by computation from the head or pressure acting on an orifice, is necessarily the velocity of the most contracted section of the issuing steam. I perceive, on reviewing my reasoning, that I did not rigorously observe the distinction between the orifice and the contraction section, but the contraction is so slight that this want of rigor can lead to no sensible error till we come to compute the discharge. Here we should make a material error if we multiplied the velocity in the contracted section by the area of the orifice. We must multiply the velocity in the contracted section by the area of the contracted section, which, in the present case, is five per cent. less than the area of the orifice.

(2.) Weisbach was one of the most skilful experimenters of his time, and one of the ablest writers, but liable to fall into a commonly received error like other people. I do not understand that my adoption of his coefficients of skin friction and efflux implies any concession as to the accuracy of his views on centrifugal force.

(3.) By the angular velocity of a rotating body, I understand the velocity of the point therein at units distance from the axis of rotation, and perceive nothing incongruous or out of place in expressing such velocities in feet per second.

[NOTE.—In the table on page 32 (J. F. I., July, 1884,) in the subcaption of column 8, instead of ω read $\omega\,c$.]

(4.) Prof. C. says that the burden of proof rests with me, I having first pronounced the ordinary theorem of centrifugal force in turbines, erroneous. This is very true, and the object of the present writing is to sustain that burden. Experiment is the court of last

resort in disputed physical questions, and I refer to the accompanying table for a comparison of my views with the results of experiment.

Nevertheless, I deem it no heavy task to maintain my position on purely theoretical grounds. The ordinary expression for the centrifugal force F, acting on a body A, of mass m, moving in a circular path of radius r, with a velocity v, is

$$F = m \frac{v^2}{r}$$

The question at issue, if a question can exist on such a matter, is: What does v mean? I affirm that it must mean the velocity of the body A, and that no correct use can be made of the formula if v is understood in any other sense. Weisbach and the other philosophers to whom I have referred in former articles, affirm that the formula will give correct results when v is understood, not as the velocity of A, but as the velocity of some other thing whose motion is not identical with that of A, and only connected therewith through some occult law of mechanics. It appears to me a waste of time to argue over such a proposition. It becomes a manifest absurdity as soon as stated.

My article of July, 1884, contained one error which has escaped Prof. Church's notice. I mention it to anticipate criticism. The wheel in question, like all turbines, had a certain amount of clearance between the fixed and revolving parts, through which some water issuing from the guide passages necessarily escaped without traversing the wheel. Of course, it is the aim of the wheel builder to make this clearance as small as possible, consistently with the proper movement of the wheel, but the loss from this source cannot be wholly avoided. In my reasonings, I assume the quantity of water traversing the guide and wheel passages as identical, and the result is theoretically, though I think not practically, affected by this slight error. Of course, the final computation of the quantity is free from direct error from this source, as it refers to the guide openings, and therefore includes all the water.

SCIENTIFIC NOTES AND COMMENTS.

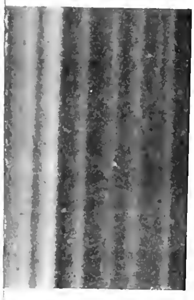
ASTRONOMY AND PHYSICS.

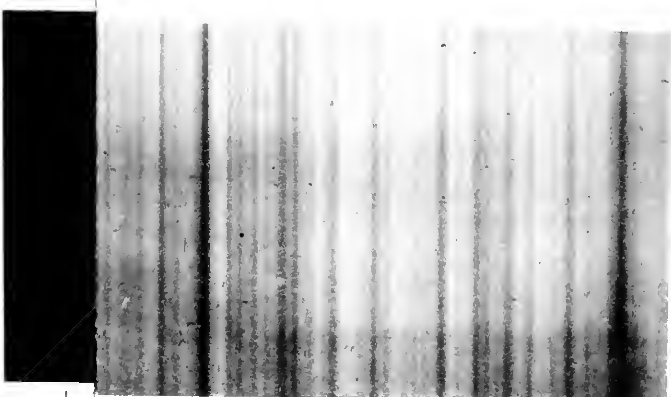
PHOTOGRAPHY OF STARS AND OF THEIR SPECTRA.—Prof. Edward C. Pickering, Director of the Harvard College Observatory, has kindly permitted the reproduction in this issue of the JOURNAL of several examples of his photographs of stellar spectra, which along with those illustrating his report on the Henry Draper Memorial, also appearing in this issue, will convey some indication of the great progress made in this direction during the last five years. As marking an important epoch in a research beset with difficulties, and yet promising results of supreme interest, these marvellous spectra deserve especial attention. The prints of the spectra are phototype reproductions of the original negative without correction.

Before more particularly remarking upon the photographs of stellar spectra, it seems proper, briefly here to refer the collateral work on the photography of the stars now being conducted at Harvard. In a paper, entitled "An Investigation in Stellar Photography," published some months since, and which is alike rich in theoretical considerations, and in description of important results attained, Prof. Pickering contributes the most valuable development of the subject now accessible. It covers the essentials in the whole range of celestial photography, and we limit ourselves to the selection of a few items of general interest.

Stellar photography originated in an experiment made at the Harvard Observatory on July 17, 1850, when, under the direction of Prof. W. C. Bond, Mr. J. A. Whipple placed a sensitive daguerreotype plate in the focus of the fifteen inch equatorial, which, driven by the clock, was kept pointed upon the star *α Lyræ*. A satisfactory image of the star was obtained, but imperfections in the driving clock, and the lack of sensitiveness of the plates prevented further progress at the time. These difficulties were partially remedied in 1857, when Prof. G. P. Bond resumed the research, and gave the results to the scientific world in three remarkable papers, now ranking as classics, and presenting nearly all the arguments at present offered in favor of stellar photography. Passing by the extended investigations undertaken in 1864, by Mr. Rutherford, and continued so successfully through many years, we find Dr. Henry Draper, after the invention of dry plates, already in March, 1881, able to obtain a photograph of the nebula of *Orion*, upon which a star of the 14·7 magnitude is shown. A few years later, A. A. Common's fine photograph of the same nebula showed many stars not hitherto mapped, although it also omitted just as many previously catalogued. Photography was now equal to the eye. We find that in several important cases, it must now be declared superior in point of sensitiveness.

The first series of experiments, by Prof. Pickering, was undertaken in 1882, with a camera of but two inches aperture, and clearly demon-





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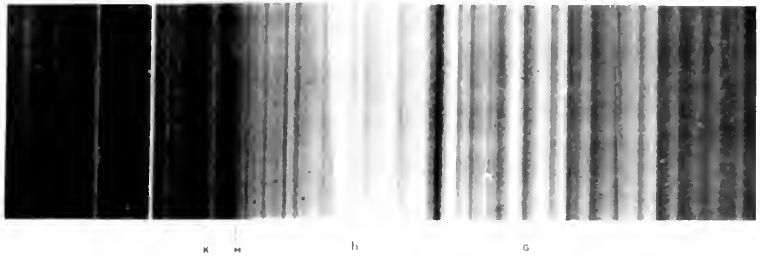
Henry Draper Memorial.



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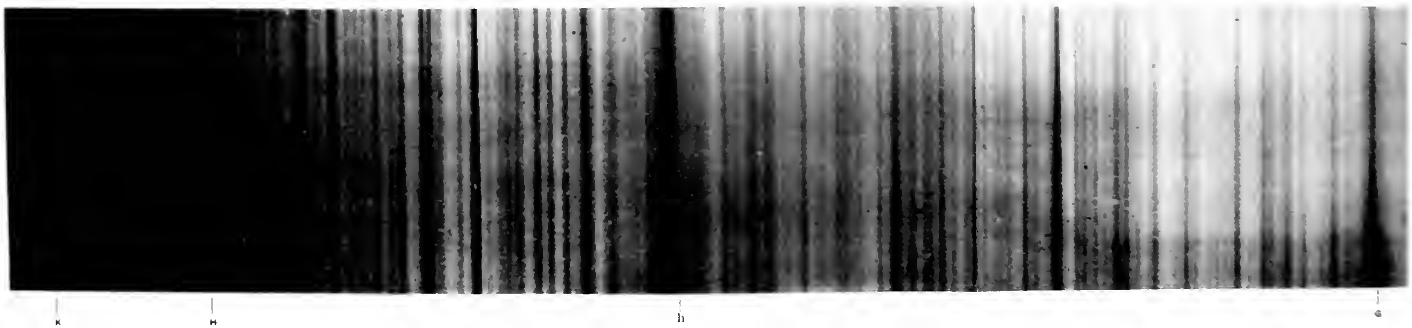


α Canis Minoris. 4 Prisms.

Phototype F. Gutekunst Philad'a

strates the power of photography as a means of forming charts of large portions of the sky, and of determining the light and color of stars in all portions of the heavens.

A second series was undertaken in March, 1885, with results that have led to the adoption of a very extended scheme of stellar photography. The instrument employed was a Voigtländer lens of 8 inches aperture and 114·6 cm. focal length, so as to make the scale of the photographs as nearly as possible 2 cm. to 1°. The plates may cover as much as 12° in declination and 10° in right ascension. With the camera at rest, stars at the pole as faint as the fourteenth magnitude and at the equator as low as the eighth magnitude leave trails. Prof. Pickering has shrewdly applied these trails to stellar photometry, and has been able to determine the photographic brightness of stars with an average deviation of less than a tenth of a magnitude; a degree of accordance greater than that given by any other photographic method. When the camera mentioned is driven by clock-work and with an exposure of plates of an hour, charts may be constructed having the same scale and dimensions as those of Peters and Chacornac. Prof. Pickering has also applied photography to the transit instrument, and from careful measures shown that the position of a star may be determined from its trail with an average deviation of $0^s{\cdot}03$, which is about one-half the corresponding deviation of eye observations.

Reference to several photographs of special interest must not be omitted: The photographic discovery of a nebula around the star *Maia*, in the *Pleiades*, by the Henry Brothers, of Paris, is well-known. A photograph of the same group, by Pickering, several weeks previously, showed the peculiarity which, though recognized, was ascribed to a defect in the plate. As showing the value of a permanent record of the condition of the sky, we have the fact that the region containing Gore's new star, presently to be further spoken of, had been photographed at Harvard five weeks earlier than the star's discovery, and that its absence from the plate shows that it must then have been considerably fainter than when first seen.

The general character of the investigations of the photographic spectra of the stars, now being conducted as a memorial to Henry Draper appears from Prof. Pickering's report, here reproduced. We should, however, mention that one of the great advantages of this novel method of photographing stellar spectra comes to light in the case of an investigation of the spectra of the *Pleiades*, where uniformity of character is clearly shown, and the few stars not possessing these characteristics are pointed out as probably only in the same line of sight, and, therefore, in the study of the parallax of the *Pleiades*, specially to be observed. As many as 100 spectra are, by this process, photographed on a single plate. It would be impossible now to divine what may not result from such photography of the spectra of stars over the whole heavens, and from the review of the work at some future date.

The spectrum of *o Ceti*, shown in the accompanying phototype, belongs to the important class of bright line spectra, and deserves especial attention from the fact that Gore's new star, near χ^1 *Orionis*, strangely enough has a spectrum of similar type. It is well known that *o Ceti* is a long period vari-

able, and because Gore's *nova* steadily decreased in brightness from the sixth magnitude at its discovery, December 13, 1885, to less than the eleventh in May, 1886; it was classed as a variable of the same type. The similarity of the new star's spectrum, as photographed by Pickering, is, however, an additional confirmation.

The spectrum of *α Canis Minoris*, as shown, includes the portion between *G* and *K* is well suited to show the great power of the new method and of the extraordinary appliances employed. When Prof. Pickering is also able to include in the same photograph some well defined reference line, the determination of the motion of stars in the line of sight must become a comparatively easy problem. This most difficult kind of measurement of the displacement of lines in stellar spectrum will thus also have received a material improvement. M. B. S.

THE INTERNATIONAL ASTRONOMICAL CONGRESS.—Accounts (*Astrom. Nach.* **116,** 384; *Nature,* **36,** 7, 54) of the Paris Conference on Stellar Photography (April 16–25, 1887) seem to show its decided success in attaining organization of the proposed work. There were present fifty nine astronomers and physicists, representing fifteen different nationalties.

The main object of this conference being the organization of a comprehensive plan for making a photographic survey of the heavens, the discussions, both in the astronomical and photographic sections, formed were almost entirely confined to the technical details of this great undertaking. With the desire that a great number of observatories should participate in the work, the conference selected instruments of moderate cost. Only refractors are to be employed; the aperture being 0·33 metre, the focal lengths about 3·43 metres, so that a minute of arc shall be represented approximately by 0·001 metre. It was decided that the photographic chart should extend to stars of the fourteenth magnitude, but that there should also be made supplementary negatives of shorter exposure, containing all stars down to the eleventh magnitude inclusive. The latter plates, on account of the sharper and more accurately measurable images, are to give the means for determining positions of stars to the eleventh magnitude with precision. All the plates to be used are to be prepared according to an identical formula, and the object-glasses of the instruments so corrected as to obtain the maximum sensibility of the photographic plates.

The conference adjourned after having delegated its powers to a Permanent Committee, consisting both of the Directors of the observatories, who may participate in the work, and of eleven members, to be elected. The following astronomers were chosen: Christie, Dunér, Gill, Prosper Henry, Janssen, Loewy, Pickering, Struve, Tacchini, Vogel and Weiss. As participants in the work, the following gave definitive adhesion before the election: Mouchez, Roget, Bailland, Trépied, Beuf and Cruls, a number of other members postponing the announcement of their participation to the near future. The Bureau of the Permanent Committee, with Mouchez as President and Gill, Loewy, Vogel as Secretaries, at a meeting April 27th, agreed to the following distribution of experimental work: (1.) Systems of cross

wires: Vogel. (2.) Photographic magnitudes: Struve and Pickering. (3.) Optical determination of images by means of photographs supplied by the Henry Brothers, Struve. (4.) The study of three or four stars nearly in a straight line, embracing the total angular distance of about 1°, and photographed necessarily at the centre, and at the corner of a plate: Paris, Algiers, Pulkowa and Leyden. (5.) Study of the deformations of films: Algiers, Meudon and Potsdam. (6.) Study of curved plates from the triple point of view of construction, means of covering with a film, and measures: Christie. (7.) Study of absolute orientation; that is to say, the mounting of the plates in the photographic telescope: The Cape and Paris. (8.) Study of the measuring instruments to be applied for the future utilization of the negatives: Postponed. (9.) The study of formulæ for preparation of plates in accordance with the general rules laid down by the Conference: Abney and Eder. (10.) Opinions of colors of stars on their photographic magnitudes: Dunér.

The institution of a series of test objects was also agreed to, and the preparation of a list of such objects assigned to Gill, Vogel and Henry. It is expected that the preliminary tests may be completed and the instruments constructed so as to be able to begin work by 1889. The congress also voted the appointment of a sub-committee, which should occupy itself with the application of photography to astronomy other than the construction of the maps, and desired that MM. Common and Janssen be charged to carry out the resolution. M. B. S.

The Morrison Observatory.—Prof. C. W. Prichett (*Publications of the Morrison Observatory*, Glasgow, Mo., **1**,) gives a description of the instrument of this observatory, the details of determination of the geographic co-ordinates and a valuable collection of miscellaneous observations. The principal instruments are an equatorial refractor by Clark, of 12¼ inches aperture and 17 feet focal length, and a transit circle by Troughten & Sims, with telescope of 6 inches aperture and a focal length of 77 inches. There are fitting accessories, and a fund, though somewhat meagre, for the support of the observations themselves. Among the observations here published are measures of 238 double stars occultations of glass and planets; two series of observations of the diameters of *Mars*; studies of the surface of *Jupiter* and the phenomena of its satellites; measurements on *Saturn*, and its system of rings and satellites; figure and dimension of *Uranus* and observations of comets. It is to be hoped that considering the excellent character of the work presented in this first volume, the wealthy west will soon endow the work of this observatory in such a manner as may allow the full capacity of the instruments to be developed. M. B. S.

New Optical Glass and Apochromatic Lenses.—Prof. E. Abbe (*Jena Ges. für Med. u. Nature*, 1886; *Journal, R. M.* **8**, 56) states some of the important results attained by himself and Dr. Schott, in the production of new kinds of glass for the use of opticians, and refers to some of the marked improvements he has thus been able to bring about in the microscope.

The researches have continued for five years, and have been supported by the Prussian Government with the final result of the establishment of the

Glastechnisches Laboratorium of Jena, where the new kinds of glass are manufactured for general use. Crown and flint glass can be produced, in which the dispersion in different parts of the spectrum is nearly proportional, so that in achromatic combinations it is now possible entirely, or almost entirely, to do away with the hitherto unavoidable secondary spectrum. Varieties of glass are produced with the mean index of refraction constant while considerable variations are given to the dispersion, or to the refracted index, while the dispersion remains constant. A high index of refraction may be retained with a low degree of dispersion.

The "apochromatic objectives" for microscopes with "compensating eye-pieces" are of the first applications. For photo-micrography these systems of lenses are specially commended. M. B. S.

Electric Induction and Transformers for Electrical Distribution.—At a recent meeting of the Institute, there was shown a small model of a secondary induction coil which had been used a few days previously in the Institute lecture course. It was intended to show, in a crude way, the general principle of induction of one current by another, and particularly the induction or generation of a current of great quantity but low pressure, by means of one of small quantity but great pressure, this being the system introduced some years ago in Europe and lately brought into this country.

The system is used chiefly for incandescent electric lighting at a distance. The currents required for such lighting being of great quantity and of low tension, the transportation of such currents, by means of wires from the generating station to the consumer, is accompanied either by a great loss of power in the mains, or else it necessitates great expense in the first cost of the mains, which, when considering the interest and depreciation, is equivalent to a running expense of no small amount. In a system of induction coils, or "transformers," as they are generally termed, the energy is transported in the form of a small current of very high tension, in which form it may be led great distances along a comparatively small conductor, without any great loss, as the size of the wire is to a certain extent proportional to the current, but independent of the pressure. Such currents, which would be useless in this form for incandescent lighting, are converted or transformed at the consumers by means of these induction coils, into currents of great quantity by low tension.

These transformers are in principle mere Rhumkorff coils reversed, the primary current being of high tension and small quantity, and the secondary the current of great quantity. In detail, the apparatus shown differed greatly from the Rhumkorff coil, which, in the usual form, would be very inefficient. It consisted of a coil eight and one-half inches mean diameter, of 127 turns of No. 16 B. & S. wire, weighing four pounds. The coil was taken just as it came off the reel, and bound with tape and shellaced canvas so as to have a circular cross-section. Next to this was placed a similar ring of very thick wire, consisting of eight turns of No. 3 B. & S. wire, weighing 3 pounds 5 ounces, the diameter of this ring being the same. These two coils were then bound together and wrapped with eleven and one-half pounds of fine, soft iron bell hanger's wire.

As the induction of currents in a neighboring wire takes place only with changes or reversals of the primary current, this primary current must either be intermittent or alternating. As the former is accompanied by destructive sparking, the latter is the only practical form of current to be used in practice.

When the apparatus was exhibited, an alternating current from a small Gerard machine was passed through a small german silver wire, No. 22, and into the fine wire coil. Upon connecting the coarse wire terminals with a similar german silver wire, it was instantly fused, showing that the same energy which passed through that fine wire in the form of a small current of high tension was transformed into one of such great quantity as to instantly fuse a similar wire. The same current was strong enough to fuse several strands of heavy iron wire, and to melt into a globule a three-inch wrought-iron nail.

Owing to the want of suitable measuring instruments, no quantitative tests could be made. As the only possible loss in such a coil is the heat developed in the copper wire due to its resistance, and the heat developed in the iron due to changes of magnetism and Foucault currents, it was quite correct to conclude that, as the coil kept quite cool, the efficiency was very high.

The generation of a current in such a coil may be readily explained on the same basis as the generation of currents in a dynamo. In the latter we know that when a wire passes through or "cuts," as it is termed, magnetic lines of force, a current will be generated in that wire. In the dynamos, the wire is moved mechanically nearer a magnet. In the transformer, as well as the Rhumkorff coil, it is the line of force or the magnetism which moves, as it might be called, across the wire. This may be understood as follows. Every current is encircled by numerous lines of magnetic force, as shown in the galvanometer and electro-magnet. These lines decrease in number as they are farther from the wire. A few years ago, the writer called attention to the fact that all the well-known laws of the mutual action and induction between currents could be explained by their lines of force. The attraction and repulsion of like and unlike currents could be explained by the attraction and repulsion of these invisible lines of force. The induction of a current in a wire, which was moved toward or away from a current, was explained by the cutting of the lines of force around the current by the moving wire. When a current is started in a wire, the lines of force, being proportioned in number to the current, will at first be very small circles around the wire, and as the current increases, these circles will grow larger; they may be said to be similar to the circular ripples produced on the surface of a still pond when a stone is thrown into the water. It is evident that a wire lying parallel to the first, and near to it, will be cut by these emanated lines of force, and, therefore, a current will be induced in it. From the laws of induction and of cutting lines of force, the direction of the induced current can be shown to be that actually observed. Similarly when the current was stopped, these lines of force came together again like a stretched rubber ring, and thereby cut the neighboring wires again, but in the opposite direction, thus inducing a current in the opposite direction. By thus considering the mutual action of currents as being merely the well-known action of the lines of force encircling the currents, the induction of currents in the Rhumkorff coil was readily explained

without the use of Ampère's laws of the mutual action of currents, or of Faraday's and Lenz's laws of induction.

The induction of currents in a transformer is, therefore, explained by the simple laws of cutting lines of force; when two or more wires lie parallel, and are encased in common by a sheathing of iron, the lines of force emanating from a primary wire when a current is started in it, cut the other wire, the secondary, in passing to the iron sheathing.

If the primary wire alone were encased by an iron sheathing, the lines of force from it would not pass outside of the iron casing; being retained by it, there would therefore be little or no induction in a secondary wire outside of the iron casing, even if quite near. This is made use of in some anti-induction wires, in which the wire which produces the induction is wound with iron tape or bands. This applies only to the primary or disturbing wire; a wire would not necessarily be protected from induction from others by being itself encased by iron; thus in the case of an electric light and a neighboring telephone wire, it is the former and not the latter which should be encased in an iron sheathing. C. H.

CHEMISTRY.

PHOTO-SALTS OF SILVER.—An exceedingly interesting account is given by M. Carey Lea in *Silliman's Journal*, of May and June, of a prolonged investigation of the haloid salts of silver and their relation to light, which may have an important bearing on the settlement of the question of the nature of the so-called latent image as well as of heliochromic possibilities, and at the same time furnish a more precise working basis for photographic operations. Without attempting a complete abstract, a few of the more salient features will serve to indicate the character and range of the investigation which the original of thirty pages alone can at all satisfactorily present. He describes in detail the formation by purely chemical means, without the aid of light, by a great variety of reactions, of haloid salts of silver, of great stability, when not exposed to the action of light, and exhibiting great variation of color, in the same haloid from rose to black. He proves the identity of these compounds with the products of the action of light upon the normal silver haloids by a series of reactions, including development, and reversing action in case of the iodide and bromide. He suggests the names photo-chloride, photo-iodide, and photo-bromide, to distinguish them from other haloids, since they were first obtained in an impure form by the action of light. According to his view, the so-called latent image as well as the visible products of the more prolonged action of light, are not sub-haloids of silver but rather a mixture, or a combination in non-stochiometrical proportions of a silver haloid with a small proportion of its sub-haloid; or possibly a mixture of the haloid with a small proportion of a stochiometrical compound of the haloid with its sub-salt. The general method for the formation of the photo-haloids is the same, but the photo-chloride is the most stable, and possesses the additional interest of a manifest tendency to the reproduction of colors, which renders it of great promise in heliochromy, obtainable thus in quantity and under

conditions more favorable to experiment than by previous methods. Among the variety of methods for producing it may be mentioned: chloriding of metallic silver; action on normal chloride by reducing agents; partial reduction of silver-oxide or carbonate by heat, and treatment with HCl; treatment of sub-oxide of silver with HCl, followed by nitric acid; action upon sub-chloride by nitric acid or an alkaline hypochlorite; action upon a soluble silver salt with ferrous, manganous, or chromous oxide, followed by HCl; reduction of an organic salt of silver, or of a soluble salt of silver by organic matter, and treatment with hydrochloric acid. C. F. H.

MANUFACTURE OF A YELLOW DYE-STUFF FROM GALLIC ACID. (From abstract in *Journal of Soc. of Chem. Indus.*, **6.**, *285*).—The dye, which can be fixed on the mordanted fibre, like alizarin, and which has been termed "galloflavin," is obtained by the action of the air—*i. e.*, oxygen—on alkaline solutions of gallic acid. The process of oxidation depends on the amount of alkali present, for, whereas, it proceeds too quickly if the alkali be used in excess, the oxidation can be moderated by diminishing the quantity of alkali.

In practice, five parts of gallic acid are dissolved in eighty parts of alcohol of 96° Tr., and 100 parts of water. The cooled solution is gradually mixed with seventeen parts of a potassium hydrate solution of 30° B., stirring all the while, and never allowing the temperature to rise above 10° C. It is then exposed to the action of oxygen, either by blowing air through it or agitating it briskly.

The progress of the oxidation shows itself by the liquor assuming at first an olive-green or greenish-brown color, until finally a crystalline precipitate separates out. When the amount of this precipitate no longer increases, the operation is finished. The mass of crystals is quickly filtered, dissolved in warm water, decomposed with hydrochloric or sulphuric acid, and boiled, when the dye is precipited in the state of glittering greenish-yellow plates. These are washed, and can then be applied for dyeing or printing. Galloflavin dyes cotton, mordanted with alumina, greenish-yellow, which turns into a very brilliant yellow by the treatment with tin crystals. The chromium-lake of galloflavin is distinguished by being especially proof against soap, air or light. H. T.

IMPROVED PROCESS FOR MANUFACTURING TANNIC ACIDS IN CRYSTALS OR GRAINS. (Dr. H. Byk. *English Patent*, 15,436).—The patentee prepares a granulated tannic acid by dissolving it in a suitable solvent, and adding five per cent. alcohol containing 0·05 per cent. glycerin. This solution is concentrated and spread to dry on sheets of zinc or other metal, from which it can then be detached in semi-transparent grains. The glycerin may be replaced by gum or carbohydrates. H. T.

THE REDUCTION OF NITROBENZOL BY ALCOHOL UNDER THE INFLUENCE OF SUNLIGHT. Ciamician and Silber (*Gazz. Chim. Ital.*, **16**, 536).—An alcoholic solution of nitrobenzol exposed to direct sunlight for some months, becomes brown, and acquires an acid reaction. It then contains aldehyde, a substance that responds to the reactions of aniline, and exceedingly small quantities of another base, which appears to be quinoline. W. H. G.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, June 15, 1887.*]

HALL OF THE INSTITUTE, June 15, 1887.

MR. JOSEPH M. WILSON, President, in the Chair.

Present, fifty-five members and six visitors.

Seven persons were reported to have been elected to membership since the previous meeting.

M. ERNEST PONTZEN, Civil Engineer, of Paris, France, was, on the recommendation of the Board of Managers, elected a Corresponding Member.

The Secretary presented the following Report of the Special Committee on State Weather Service, viz.:

The Special Committee appointed by the President of the INSTITUTE, under authority of a resolution of the INSTITUTE, adopted at the Stated Meeting in October, 1886, to formulate a plan for the State Weather Service respectfully report:

That in execution of the duties imposed upon it, the Committee promoted the passage of an Act of the Legislature appropriating $3,000 for the purpose contemplated.

The Committee has also secured the co-operation and assistance of the principal railroads in the State which, recognizing the public nature of the service and the great benefits it is capable of rendering the people of the State have granted free passes to Sergeant Townsend of the United States Weather Bureau, while on duty for State Weather Service.

He has already visited a number of points of influence, particularly some institutions of learning, and has had most gratifying success, receiving welcomes, sympathy and assurances of co-operation.

He has also made arrangements for displaying signals with numerous parties on the lines of the telegraph.

This account of the work already performed will best indicate the plan which the Special Committee recommends, and having thus accomplished the object of its appointment, the Committee respectfully asks that the further prosecution of the work be committed to the Standing Committee on Meteorology, and that the Special Committee be discharged.

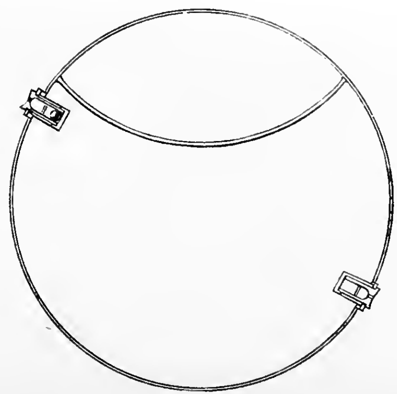
W. P. TATHAM, *Chairman.*

The Report was adopted, and the Special Committee was discharged.

Sergeant T. F. TOWNSEND, of the United States Signal Service, gave an account of the plans contemplated by the Committee in carrying on the work of the service.

Prof. M. B. SNYDER, of Philadelphia, gave an account of the experiments of Prof. E. C. PICKERING, of Cambridge, Mass., in Stellar Spectroscopy. Referred for publication.

Adjourned. WM. H. WAHL, *Secretary.*



Article: *Townsend.* "Oil for Stilling Waves," etc. This engraving was inadvertently printed upside down in the July issue. Subscribers may paste the accompanying print of same over the other in correct position. W.

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JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CXXIV. AUGUST, 1887. No. 2.

REPORT OF THE SPECIAL COMMITTEE APPOINTED OCTOBER 20, 1886, TO INVESTIGATE THE PROTEST OF POUL LA COUR AGAINST AN AWARD OF THE ELLIOT CRESSON MEDAL TO PATRICK B. DELANY FOR HIS SYNCHRONOUS MULTIPLEX TELEGRAPH SYSTEM.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, May 1, 1887.

At the stated meeting of the FRANKLIN INSTITUTE, held September 15, 1886, a communication was received from Poul La Cour, of Copenhagen, Denmark, dated July 7, 1886, protesting against the award of the Elliot Cresson medal to Patrick B. Delany for his synchronous multiplex telegraph system, on the ground that he had himself invented the system, and that he had taken out patents on it in other countries, but had disposed of his right to do so in the United States, and that his patents ante-dated those granted to Delany.

A copy of this protest was forwarded to Delany, who replied

from Berlin, under date of October 1, 1886, and at the stated meeting of the INSTITUTE, held October 20th, both communications were referred to this special committee with the request to investigate and report upon the merits of La Cour's claim. The members of the committee, although quite familiar with the history of the invention, deemed it best to go over the whole ground carefully to see if any additional facts could be found to change the opinion expressed in the report of the special committee appointed in 1884, upon whose report the award to Delany was made. Inquiries were therefore addressed to the leading telegraph officials of Europe to ascertain what, if anything, was known of La Cour's apparatus prior to the date of the patents issued to Delany and up to the present time. Answers were received from the government directors of telegraphs of Denmark, France and Germany, and their replies together with the communications of Messrs. La Cour and Delany, form a part of the report of this committee. It will be noted that only one of these replies mentions La Cour, although the communication of the French director gives a very interesting history of synchronous multiplex telegraphs in France.

The ground taken by the committee, appointed in 1884, was that La Cour was the inventor of the phonic wheel, upon which, as a basis, Delany founded his system of multiplex telegraphy. The INSTITUTE accepted that view, and, at the suggestion of one of its members, awarded the Scott Legacy Medal to La Cour for that invention. No credit was given La Cour for any device to produce synchronism between two phonic wheels, because the committee were perfectly well aware that not even approximate synchronism had been or could be produced by his apparatus; that after long and unwearied efforts by the most competent experts in America, his method, as described in his patent, was found to be impracticable; also, that La Cour himself, who came to this country, in 1881, to demonstrate the efficiency of his apparatus to the parties to whom he sold his rights in the phonic wheel, failed utterly to produce synchronism by his device.

In the report of the committee of 1884, the fact was emphasized that the essential feature of Delany's system was its practically perfect synchronism, and that without this multiplex telegraphy, by means of the phonic wheel, would be impossible; with it many other methods of telegraphy were possible, including autographic writing.

La Cour saw that the phonic wheel could be made a valuable factor in a multiplex telegraphic system, if he could only produce synchronism between the two wheels. If the specifications in his patents, issued prior to the patents of Delany, do not describe a practicable way to do this, then he has no claim to be the inventor of a synchronous system. It is a general principle, well established in regard to patents, that the description shall be so plain that a person, skilled in the art pertaining thereto, will be able to accomplish the same results as are claimed for the apparatus by the inventor. Now, the proof is abundant that neither by La Cour's system, invented, as he says, in 1881, and which he claims is the same as Delany's, nor by his "improved plan," of October 7, 1882, has any American electrician been able to produce a movement of the two wheels which approximates synchronism so nearly that multiplex telegraphy can be accomplished by it.

The claim is made by La Cour that two circuits over one wire were successfully worked in opposite directions by means of his synchronism, based upon his phonic wheel, between Fredericia and Nyborg, Denmark, a distance of forty miles, in June, 1880. Let us see! In response to an inquiry addressed to the Managing Director of the Danish Government Telegraph, to whom La Cour refers, the following letter was received:

Direction
des Télégraphes d'Etat
de Danemark.

COPENHAGEN, February 9, 1887.

Dear Sir:—In reply to your letter of January 24th, I have the honor to inform you that in the year 1880, during the twenty-four days from the 17th June to 10th July, arrangements were made on the telegraph offices of the Danish Government, at Fredericia and Nyborg, for sending two messages simultaneously over one wire by means of Morse apparatus and separate circuits produced by "phonic wheels," invented by Poul La Cour as synchronous instruments.

I regret that I have not seen the experiments myself, but I am in possession of a diary, held by an officer of the Danish Telegraphs, whereby it is stated that during that time, from the 17th June to the 10th July, 1880, the "phonic wheels," in Fredericia and Nyborg, were working every day from 8 or 9 in the morning to 9 in the afternoon, only with a few short interruptions caused by several events, such as disturbances on the telegraph wire, or mechanical disturbances on the "phonic wheels." For the period from the 22d June to the 10th July, it is expressly remarked that the synchronism was "good."

I am further in possession of official reports, dated 28th June, 1880, from

the head officers of the telegraph offices at Fredericia and Nyborg, after which the apparatuses on the both offices have been working regularly, and messages have been sent since the 17th of June, except two days when the working of the "phonic wheels" was disturbed by mechanical causes. In these reports it is especially stated that on the 27th of June, 1880, were sent *over one wire*; from 9 hours 57 minutes, to 10 hours 19 minutes simultaneously, four messages on each of the two circuits from Fredericia to Nyborg, eighty words on the one and seventy-seven words on the other circuit. From 10 hours 20 minutes, to 10 hours 36 minutes, simultaneously, four messages on each of the two circuits, from Nyborg to Fredericia, sixty-eight words on the one circuit and sixty-three on the other. From 10 hours 48 minutes, to 11 hours 2 minutes, simultaneously, four messages (sixty-eight words) from Nyborg to Fredericia, and four messages (sixty-five words) from Fredericia to Nyborg.

These messages, as well as the paper slips, such as received at the office of Fredericia, are in my hands.

After this, there is no doubt, that in the year 1880, in this country, messages have been sent simultaneously over one wire by means of separate circuits produced by the "phonic wheels," invented by Mr. Poul La Cour. Before that time there was not, nor has there ever since been any telegraph line in this country operated by synchronism as a multiplex system.

With great respect, I remain, very truly yours,

[Signed.] —— ————— *

Director of the Danish Government Telegraphs.

MR. E. ALEX. SCOTT, Chairman of Committee of the FRANKLIN INSTITUTE, Philadelphia.

In the first test, lasting for twenty-one minutes, eighty words were sent on one circuit and seventy-seven on the other circuit, or an average of less than four words per minute on each circuit; or, say, eight words per minute for the whole wire The second test gave about the same average, and the third test a slightly larger one—say, nine words per minute for the whole line.

These experiments were made at the home of La Cour, and presumably under his supervision, and it is fair to assume that the record made was of the best performance of which the system was capable. What do the tests show? Why, most conclusively that there was no synchronism nor even an approximate synchronism. This committee is familiar with the fact that a single wire with one circuit is equal to the transmission, by the Morse system, of from forty-five to forty-eight words per minute, the limit of the ability of expert operators to manipulate the key; and that the same wire, if duplexed, is equal to the transmission of twice as many words,

* Signature illegible. (W.)

and the multiplex system of Delany of six times as many words. It appears, however, that the record of the best performance of La Cour's apparatus shows but nine words per minute for both of the so-called circuits, only one-fifth of what the wire was capable of performing as a single circuit, or one-tenth of what it ought to have accomplished with duplex circuits. It shows that the words actually sent and received were caught by the lucky operator during the brief moments when the instruments had drifted together and before they had separated again; and it may be remarked here, that the more they were out of synchronism the oftener they would drift together.

This is the only reference La Cour has given the committee of a successful working of any system of his where the telegraphing was done supposably by Morse signals. But he says in his communication: "This method being somewhat different from the one now in question, I shall say no more about it," so that we have nothing to do with the only method of his, for which he claims successful results in Morse telegraphy. What we are to look to then, are his later patents, prior to the date of the patents taken out by Delany. What are they?

He says he invented in April, 1881, the system claimed by Delany; that in September, of that year, he sold his patent for the phonic wheel to Mr. Jones, with the right to use any of his later patents; that he took out a patent in England, October 7, 1882, which agreeably to his contract with Jones, he sent to him October 1, 1882. It is this patent marked in La Cour's communication as "Exhibit A," with which La Cour requests the INSTITUTE to compare Delany's synchronism, Delany's descriptions being, as he says, only immaterial modifications of it. A later communication from La Cour to the INSTITUTE, dated March 12, 1887, again refers to this patent as describing a perfectly-working synchronism.

It is a fact, well-known to the committee, that several expert electricians in America, including Delany, Calahan and Yeakle, were unable to produce synchronism by the means described in the patent. Let us see why.

Referring to the drawing accompanying La Cour's patent of October 7, 1882, [*Fig. 1*,] at Station 2, the fork is kept vibrating by a local battery, whose circuit is broken by the fork's vibrations, and the

current is therefore intermittent. The rapidity of the vibrations is decreased by a stronger local battery being substituted for the weaker, the principle being that the wider apart the arms of the fork are drawn asunder, the slower will be its vibrations. La Cour, therefore, directs that the fork at Station 2 be tuned so that its phonic wheel shall turn faster than at Station 1, the fork being operated by the weaker battery. Its phonic wheel, therefore, commences to run faster, and the trailing arm gets ahead of the arm at Station 1, so that a positive current is sent through the line which actuates the polarized relay *R*, and throws the stronger local battery into the circuit. The two wheels are now running apart; they have been running apart all the time, but getting further and

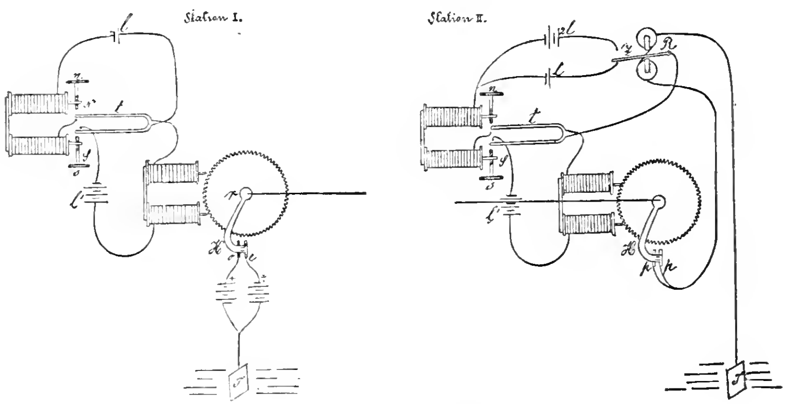


FIG. 1. La Cour's Method.

further apart, because No. 2 has been constantly gaining. No. 2 has run faster, because its local battery was too weak; before, however, it commences to feel the effect of the stronger local battery, which will tend to check its speed, it is left without any battery at all, while the armature of the correcting relay passes from one stop to the other; this gives it a still greater speed and puts the two wheels still further apart. When the stronger battery is substituted, this process is reversed; No. 2 commences to run slower, and as the wheels are now widely apart, the battery must be strong enough to make it run slower than No. 1. No. 1 finally overtakes it, and for a brief moment the two wheels are synchronous. But as No. 2 is now running slower than No. 1, it falls behind it, and the two wheels drift apart again until a negative

current is sent through the line, which throws the armature of the polarized relay to the other stop and puts on the weaker battery. Thus, as La Cour states in his patent: "The relay will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1."

It is hardly necessary to say that this is *not* synchronism. The wheels are *compelled* to be out of synchronism all the time. It is not even close enough to be approximate synchronism, and it would be impossible to use such a system even for duplexing a line, to say nothing of creating a greater number of circuits.

Delany having found La Cour's method impracticable, abandoned it, and set about constructing a new system by which each wheel kept the other from running irregularly, not by bringing it back after it had gotten out of synchronism, but by preventing it from getting out, the slightest tendency to go out being immediately checked. This was done by the introduction of resistances into the fork circuit, which, being instantaneously introduced, operated immediately. This was a radical change in three particulars: (1), in the mutual correction by the two wheels of each other's movements; (2), in the application of the correction before the wheels were out of synchronism, and (3), by the instantaneous introduction of *resistances* into the circuit.

The especial object of La Cour's letter of March 12, 1887, was to call attention to the fact that G. A. Cassagnes, a French engineer, successfully operated his synchronous autographic system, which he calls steno-telegraphy, by means of his (La Cour's) synchronism as described in La Cour's patent of October 7, 1882. The Committee found in the Memorial Library of the INSTITUTE, a copy of a pamphlet issued by Cassagnes in 1886, descriptive of his system of steno-telegraphy, which states that the system is based upon the synchronism produced by La Cour's phonic wheel. Drawings of the apparatus are given in the pamphlet which show *not* La Cour's system of two batteries of different strength, but Delany's method of instantaneous introduction of resistances. (See *Fig. 2*, page 88,)

It is unnecessary to enumerate the many minor details required to produce the results effected by Delany. Suffice it to say that, having perfect synchronism between the two wheels when standing side by side, that alone, was not sufficient to produce multiplex

telegraphy, as Delany found to his cost when he put his apparatus to a practical test on a telegraph line. A new problem awaited him. After the passage of every current of electricity over the line, the static charge of the line discharged itself through the succeeding segment, producing an effect similar to that produced by the battery current. Signals, therefore, sent on one segment were received on two or more.

To dispose of, or neutralize, or utilize this current was the task which Delany had now to undertake. La Cour ignores it in his

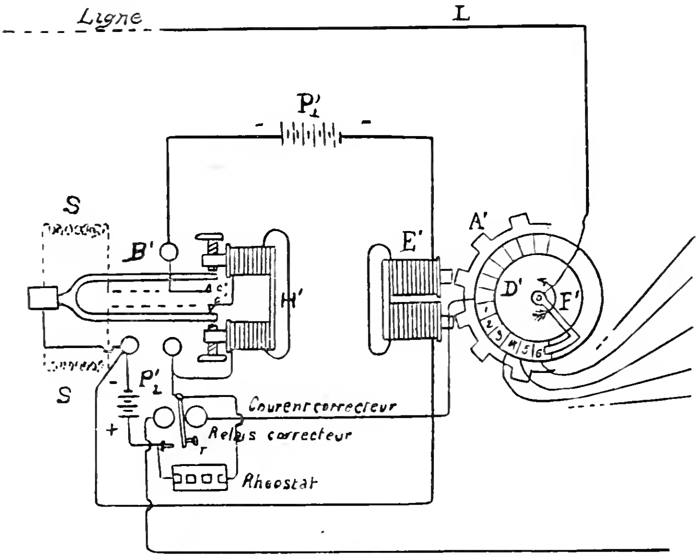


FIG. 2. From Cassagnes's Pamphlet.

patents: he seems never to have gotten far enough with his investigations in multiplex telegraphy to have encountered this difficulty, and yet it is one of the most perplexing problems with which the telegraph engineer has to deal.

The INSTITUTE is familiar with the method by which Delany disposes of this static charge by the introduction of alternate ground segments between the line segments, and it is unnecessary for the purposes of this report to show how the Delany system has reached its present condition, surpassing as it does all known systems of Multiplex Telegraphy. It is sufficient to know that without synchronism, which La Cour did not have, and without discharging, or in some way neutralizing, the static charge of theline,

which La Cour did not attempt to do, there is no such thing as multiplex telegraphy by means of the phonic wheel.

The Committee beg leave to say that in their opinion, full justice was done to La Cour in the award to him of the Scott Legacy Medal and Premium for the invention of the phonic wheel; but that to Delany belongs the credit of inventing and perfecting a highly successful synchronous system of multiplex telegraphy based upon the phonic wheel.

The Committee therefore respectfully report, that they find no substantial reason for revoking the award of the Elliot Cresson Medal to Patrick B. Delany, already approved by the INSTITUTE.

[SIGNED.]

E. ALEX. SCOTT, *Chairman.*
W. W. GRISCOM,
EDWIN J. HOUSTON,
A. E. OUTERBRIDGE, JR.,
WM. H. WAHL.

APPENDIX.

Contents.

(1.) Protest of Poul La Cour with Patent Specification of Frederick Wolff.

(2.) Reply of Patrick B. Delany to La Cour's Protest.

(3.) Letter from the Director of Danish Government Telegraphs.

(4.) Letter (and translation) from the Minister of the French Post Office and Telegraphs.

(5.) Letter (and translation) from the Imperial German Postal Department.

(6.) Letter from Poul La Cour, accompanied by documents relating to M. Cassagnes's system of steno-telegraphy.

(7.) Letter of the Secretary of the FRANKLIN INSTITUTE to Poul La Cour.

APPENDIX 1.

(*La Cour.*)

COPENHAGEN, July 7, 1886.

TO THE FRANKLIN INSTITUTE, Philadelphia:

The most honorable INSTITUTE has shown me the courtesy to recommend the city of Philadelphia to assign me to the John Scott Medal with the attached premium for my invention of the phonic wheel.

In cordially thanking the INSTITUTE for the attention thus created for my invention, I beg leave to make the honorable INSTITUTE acquainted with, that the synchronic system which Mr. Patrick Delany has placed before the INSTITUTE as his system, and which the committee of the electrical section has spoken so favorably about, as appears from the JOURNAL OF THE FRANKLIN INSTITUTE, April, 1886, is invented by me. But having, as will be

shown below, renounced upon the right of a United States patent, the honorable committee could not be aware of, that I had the patents in other countries long before Mr. Delany took out a patent in the United States.

To illustrate the matter perfectly, it will be necessary to discern between the three things:

(1.) The invention of the phonic wheel.

(2.) The invention of the absolute synchronic movements by aid of the phonic wheel.

(3.) The applications of the synchronism for telegraphy of different kinds.

Mr. Delany does not attempt to claim the invention of the phonic wheel, and the honorable INSTITUTE has further confirmed this by the recommendation to the John Scott Medal, as is also expressed in the aforesaid report of committee (JOURNAL OF THE FRANKLIN INSTITUTE, April, 1886, page 313).

Regarding items (2) and (3), the committee says that item (2) is the main point.

It runs as follows, page 318: "The committee recognizes the fact that the practically perfect synchronism attained by this invention is a most important advance in the art of telegraphy. With synchronism many other things are possible, and it seems quite probable that its uses will not long be confined to the two systems of telegraphy herein described or even to telegraphy alone." The only question is then about the priority of the invention of the synchronism, and to that effect I beg to substantiate.

(1.) That a perfectly accurate automatic synchronism of a somewhat different system, based upon the phonic wheel, existed several years before Mr. Delany engaged himself about it.

(2.) That actually the same synchronism described in the report of the committee was invented and published by me, prior to Mr. Delany bringing his invention before the public, and

(3.) That Mr. Delany has not invented this synchronism himself, but has had immediate admittance to a descriptive information from me about the method.

I take it for granted that Mr. Delany's first publication is a United States Patent, taken by Calahan and Delany, dated 17th July, 1883, as this patent refers only to my patent for the phonic wheel formerly taken, and not at all to any prior works by Calahan and Delany.

I have therefore to prove that my inventions and publications are of earlier dates.

In June, 1880, was in use for some time incessively from morning till night, a synchronism based upon the phonic wheel, between Fredericia and Nyborg. The Managing Director of the Danish Government Telegraph, has given a testimony to show that this synchronism worked perfectly accurate and reliable, and was easy to manage. But this method being somewhat different from the one now in question, I shall say no more about it. It shows, however, that the inventions since 1880, can only be termed "improvements," not in accuracy but by regulating the synchronism by the telegraph lines in the shortest space of time possible, and thus the line can transmit a greater portion of telegraphy.

In April, 1881, I invented the system of synchronism which Mr. Delany has published as his, but I did not take out patents at once. After having sold, in September, my United States patent for the phonic wheel to a Mr. Jones, to whom I promised to communicate all further improvements with the right of taking out patents in the United States, I took a patent in England, 7th October, 1882. Description of this I sent to Mr. Jones, as promised, on 1st October, 1882, and a letter from him dated 8th November, 1882, proves that he has received it, as also another letter from Mr. Jones, in which he says: "Delany and Calahan both are working in harmony with my interests here."

Mr. Delany, I suggest, must have had immediate access to my description, and in the course of 1883 (specially 9th October, 1883), he took out a number of patents of different modifications of the same invention.

I enclose my patent for England, dated 7th October, 1882 (see Exhibit A), and the most honorable INSTITUTE will, by closer examination, find that Mr. Delany's descriptions are only immaterial modifications thereof, without any theoretical or practical importance.

My synchronism is maintained by letting the time of oscillation of the tuning fork vary, as said in my description to patent, viz.: The time of oscillation can be lengthened by:

(1.) Moving the attractive poles nearer to the branches of the tuning fork, or

(2.) By letting a stronger current act upon the tuning fork to keep it in motion.

The first alternative is specially adapted for pan-telegraphy, as the receiving telegraph clerk must regulate the rapidity himself in order to get exact copies.

The second alternative, on the contrary, makes the synchronism automatic, as a corrective current acts upon relays, and thereby letting a stronger or weaker local current act upon the tuning fork according to its oscillating too quick or too slow.

Every electrician knows that a current can be make stronger or weaker in different ways.

It is quite indifferent whether this be done by the relays letting in different resistances in the current or a different number of batteries.

Mr. Delany has, however, not been successful in his choice, as it is not necessary to make use of six contact pieces for each rotation of the wheel, only for preserving the synchronism.

In Denmark, Paris, Vienna, etc., where I have done the arrangements, two, even very narrow pieces, have been quite sufficient for the synchronism, and the rest have been available for telegraphy.

Further, it is of little or no importance to make special arrangements for proving when the synchronism is obtained, as it puts itself right and *cannot unorder* itself, as was evident by my arrangements at the Exhibition, Vienna, August–October, 1883, and is further proved by the practical use of my synchronism by Mr. Cassagnes's steno-telegraph between Paris–Marseilles.

Mr. Delany's description adds nothing particularly new to what is said in my patent of 7th October, 1882, but only serves to complicate the system which is perfectly reliable in the simplified form.

Always ready to give any desired information, I solicit that the most honorable INSTITUTE will give this publicity. I remain, gentlemen,

Yours respectfully, POUL LA COUR

I have examined the documents referred to by *stars* in this letter, and find they have been correctly cited.

B. B. ANDERSON, *U. S. Minister.*

Copenhagen, Denmark, July 8, 1886.

(*La Cour.*) EXHIBIT A.

Specification of Frederick Wolff.

[A. D. 1882, 7th October, No. 4,779.]

OBTAINING SYNCHRONOUS MOVEMENTS.

LETTERS-PATENT to Frederick Wolff, of the International Patent Office, Copenhagen, Denmark, Patent Agent for an invention of IMPROVEMENTS in OBTAINING SYNCHRONOUS MOVEMENTS AND APPARATUS THEREFOR. A communication to him by Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark.

PROVISIONAL SPECIFICATION left by the said Frederick Wolff at the office of the Commissioners of Patents on the 7th October, 1882.

FREDERICK WOLFF, of the International Patent Office, Copenhagen, Denmark, Patent Agent, "improvements in obtaining synchronous movements and apparatus therefor" (a communication from Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark).

An apparatus for obtaining synchronous movement according to this invention, consists principally of a vibrating body, such as a tuning fork, permanently vibrating or oscillating by automatic electric intermittent action, and thereby transmitting an intermittent or undulating electric current, and of a wheel subject to oscillation or vibration under the influence of the said current. The rotation of this wheel, under these circumstances, being exceedingly regular, the current is applicable to various purposes, such, for example, as to establish synchronous movements at two or more stations connected by a telegraphic line.

The more the oscillating or vibrating body (hereinafter denominated a tuning fork), is attracted, the longer is the period or rate of its oscillation and the slower does the wheel consequently move.

Thus the time of oscillation can be lengthened, for instance by—

(1.) Moving the attracting poles nearer to the branches of the tuning fork, or

(2.) By letting a stronger current act upon the tuning fork to keep it in motion.

The former can be done simply by applying through the poles N and S of the electro-magnet two screws of iron forming polar extensions.

When these are screwed nearer to the branches of the fork or farther therefrom, the time of vibration or oscillation of the fork will accordingly be

somewhat prolonged or shortened, and the wheel will consequently turn somewhat slower or somewhat quicker.

This method of regulation can be adapted to apparatus at various stations, connected by a telegiaphic line, when it is easy to tell if the movements are synchronous.

If the synchronous movements are used, for instance, to produce pan-telegraphy, it can directly be observed at the receiving station if the writing or the drawing stands correctly, or, still better, if a vertical line on the original for instance, along the border of the same, appears as a vertical line at the receiving station.

If the line is reproduced in an oblique position, bending to the one or to the other side, it proves that the revolution of the phonic wheel is too quick or too slow.

The screws forming the polar extensions can then be regulated, until the copy of the vertical line also becomes vertical.

By the latter of the above mentioned methods for altering the time of oscillation of the fork, namely, by varying the strength of the current, which causes the fork to oscillate, one can obtain absolute synchronous movements in the following manner.

At Station No. 1, [see *Fig. 1*, page 86,] the fork is kept in automatic movement by a local battery.

The circuit of a second local battery is thereby intermittently completed, and the phonic wheel can thus be kept in uniform rotation, one tooth passing at each oscillation or vibration of the fork. At Station No. 2, the fork is in a similar way kept in automatic movement by means of the local battery.

Thereby the current in a second local battery becomes intermittent and the phonic wheel has a corresponding speed of rotation.

At Station No. 2, there is also a third local battery, stronger than the first, and capable of being thrown into circuit in place of the first by the tongue of a polarized relay.

The fork is now tuned at Station No. 2, by means of the polar extensions, so that the phonic wheel at No. 2, turns quicker than at No. 1, when the fork at No. 2 is operated upon by the first local battery, whilst the wheel at No. 2, turns slower than the wheel at No. 1, when the fork at No. 2 is operated upon by the third local battery.

To each of the wheels is attached an arm which is in electric connection with the telegraphic line.

The arm at Station No. 1, touches at each revolution two contact pieces, which are in connection respectively with the negative and positive poles of two batteries, the other poles of which are conducted to the earth.

Consequently there is produced at each revolution a negative and positive undulation of short duration. At Station No. 2, there are two corresponding contact pieces, from which a circuit passes through the convolutions of the relay to the earth.

On putting both wheels in motion and placing the tongue of the relay in such a position that the fork at No. 2 is operated by the first battery, the wheel at Station No. 2 will turn quicker than the wheel at No. 1.

Thereby the arm at No. 2 will gradually overrun the arm at No. 1, so that the former touches its contact pieces at the same moment as the latter touches its positive contact piece.

The tongue of the relay stands still, however, until the arm at No. 2, after some more revolutions, touches its terminal in the very same moment that the arm at No. 1 touches the negative terminal.

The negative current wave will throw the tongue of the relay against the other contact, and thus the third battery is substituted for the first, whereby the wheel at No. 2 turns slower. After some revolutions, the arm at Station No. 2 will again touch the contact at the same moment the positive wave from No. 1 arrives to move the relay tongue, whereupon the first battery is again replaced, and so on.

Thus the relay will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1, and for that purpose the telegraphic line is only in use during the short time the arms at the two stations are passing respectively over the contact pieces.

During the rest of the time, the telegraphic line can be used for various kinds of telegraphy, the arms being available during the remainder of their motion to send and receive different currents from No. 1 to No. 2, or from No. 2 to No. 1.

Or, there can during some fractional parts of the revolution be sent currents in one direction, and during others in the opposite direction.

Thus the synchronous movements can be utilized in different kinds of telegraphy.

SPECIFICATION in pursuance of the conditions of the Letters-Patent filed by the said Frederick Wolff in the Great Seal Patent Office, on the 3d April, 1883.

FREDERICK WOLFF, of the International Patent Office, Copenhagen, Denmark, Patent Agent. IMPROVEMENTS IN OBTAINING SYNCHRONOUS MOVEMENTS AND APPARATUS THEREFOR (a communication to me by Poul La Cour, of Askovhus, Vejen Station, in the Kingdom of Denmark.)

An apparatus for obtaining synchronous movements, according to this invention, consists principally of a vibrating body, such as a tuning fork permanently vibrating or oscillating by automatic electric intermittent action, and thereby transmitting an intermittent or undulating electric current, and of a wheel subject to oscillation or vibration under the influence of said current. The rotation of this wheel, under these circumstances, being exceedingly regular, the current is applicable to various purposes, such, for example, as to establish synchronous movements at two or more stations connected by a telegraph line. The more the oscillating or vibrating body (hereinafter denominated a tuning fork) is attracted, the longer is the period or rate of its oscillation, and the slower does the wheel consequently move. Thus the time of oscillation can be lengthened, for instance, by (1), moving the attracting poles nearer to the branches of the tuning fork, or (2), by letting a stronger current act upon the tuning fork to keep it in motion.

The former can be done simply by applying through the poles *N* and *S* of the electro-magnet two screws of iron forming polar extensions. When these are screwed nearer to the branches of the fork, or farther therefrom, the time of vibration or oscillation of the fork will accordingly be either a somewhat prolonged or shortened, and the wheel will consequently turn somewhat slower or somewhat quicker. This method of regulation can be adapted to apparatus at various stations, connected by a telegraphic line, when it is easy to tell if the movements are synchronous. If the synchronous movements are used, for instance, to produce pan-telegraphy, it can directly be observed at the receiving stations if the writing or the drawing stands correctly, or, still better, if a vertical line on the original, for instance, along the border of the same appears as a vertical line at the receiving station. If the line is reproduced in an oblique position, bending to the one or to the other side, it proves that the revolution of the phonic wheel is too quick or too slow. The screws forming the polar extension can then be regulated, until the copy of the vertical line also becomes vertical.

By the latter of the above mentioned methods for altering the time of oscillation of the fork, namely, by varying the strength of the current, which causes the fork to oscillate, one can obtain absolute synchronous movements in the following manner.

Referring to the accompanying sheet of drawings, at Station No. 1, the fork *t* is kept in automatic movement by a local battery 1. The circuit of a second local battery 1^1 is thereby intermittently completed and the phonic wheel *r* can thus be kept in uniform rotation, one tooth passing at each oscillation or vibration of the fork. At Station No. 2, the fork *t* is in a similar way kept in automatic movement by means of the local battery 1.

Thereby the current in a second local battery 1^1 becomes intermittent and the phonic wheel *r* has a corresponding speed of rotation. At Station No. 2 there is also a third local battery 1^2 stronger than the first 1^1 and capable of being thrown into circuit in place of the first by the tongue of a polarized relay *R*. The fork *t* is now turned at Station No. 2 by means of the polar extensions *N* and *S* so that the phonic wheel at No. 2 turns quicker than at No. 1, when the fork at No. 2 is operated upon by the first local battery 1, whilst the wheel at No. 2 turns slower than the wheel at No. 1 when the fork at No. 2 is operated upon by the third local battery 1^2. To each of the wheels is attached an arm *H* and *H* which is in electric connection 'with the telegraphic line. The arm *H* at Station No. 1 touches at each revolution two contact pieces *e* and *o* which are in connection respectively with the negative and positive poles of two batteries, the other poles of which are conducted to the earth *T*. Consequently there is produced at each revolution a negative and positive undulation of short duration. At Station No. 2 there are two corresponding contact pieces *p* and *p*, from which a circuit passes through the convolutions of the relay *R* will cause the wheel at No. 2 to move on the average just as quickly as the wheel at No. 1, and for that purpose the telegraphic line is only in use during the short time the arms *H* and *H* at the two stations are passing respectively over the contact pieces *e o* and *p p*. During the rest of the time the telegraphic line can be used for various kinds

of telegraphy, the arms *H* being available during the remainder of their motion to send and receive different currents from No. 1 to No. 2, or from No. 2 to No. 1.

Or there can during some fractional parts of the revolution be sent currents in one direction, and during others in the opposite direction.

Thus the synchronous movements can be utilized in different kinds of telegraphy.

Having described the nature of the said invention and the manner of carrying it into practical effect as communicated to me as aforesaid, what I claim is:

(1.) Utilizing the variable attraction between an electro-magnet and a permanently vibrating or oscillating body, such as a tuning fork for the purpose of altering the period or rate of vibration or oscillation of the said body the greater attraction giving the slower rate of vibration or oscillation substantially as described.

(2.) Varying the velocity of rotation of the phonic wheel by altering the distance between the polar extensions of the electro-magnets and the branches of the fork; or by varying the strength of the current that operates the tuning fork *substantially as described.*

(3.) Regulating the velocity of rotation of the phonic wheel at the receiving station, by means of a current from the transmitting station acting through a relay, whereby the strength of current operating the tuning fork at the receiving station, is changed as required to render synchronous the rotation of the phonic wheel at the two stations substantially as described.

In Witness Whereof, I, the said Frederick Wolff, have hereunto set my hand and seal this twenty-seventh day of March, in the year of our Lord one thousand eight hundred and eighty-three.

FREDERICK WOLFF. [L. S.]

APPENDIX 2.

(*Delany.*)

103 *A* Potsdamer Strasse,
BERLIN, Prussia, October 1, 1886.

To the FRANKLIN INSTITUTE:—Through the courtesy of your honorable Secretary I have been furnished with a copy of a communication, addressed to the FRANKLIN INSTITUTE, by Mr. Poul La Cour, disputing my claim to the invention of practical synchronism and synchronous multiplex telegraphy, for which invention the FRANKLIN INSTITUTE has highly honored me by awarding me the Scott Legacy and Elliot Cresson Medals. I take the earliest opportunity to reply briefly to Mr. La Cour's communication, and at the same time to assure your honorable body that if any further explanation or information is desired to prove the correctness of my statements, I hold myself in readiness to furnish the same, or to prove by actual demonstration on my return to America that the FRANKLIN INSTITUTE has not been mistaken in its generous award.

Mr. La Cour's American patent for "Improvements in Isochronous and Synchronous Movements for Telegraphic and other Lines" bears date May 7, 1878. Its history, so far as I have been able to ascertain, is that it was first placed in the hands of a Mr. Yeakle, of Baltimore, a practical telegrapher and electrician, who was unable to make it work. Later it was submitted to the Western Union Company's electricians in New York, Mr. D'Infreville and others, who pronounced it impracticable. Still later, it was submitted to Messrs. G. S. Mott and James G. Smith, well-known electricians, practical telegraphers and inventors, who also pronounced it impracticable. It was then given in charge of Mr. E. A. Calahan, of New York, also a well-known practical telegrapher and inventor, who experimented with it for nearly a year, with no better success than his predecessors. Some time in 1881 the parties interested in the patent brought Poul La Cour, the inventor himself, from Denmark to New York, for the purpose of obtaining from his patent some of the results claimed for it, but so far as I have been able to learn, Mr. La Cour failed utterly in his mission, and returned home without having obtained synchronism.

In the winter of 1881–82, my attention was first called to the invention by Mr. Calahan, at whose urgent request I became interested in an effort to obtain synchronism from it, the understanding being that as soon as synchronism was obtained, parties interested would purchase the patent from La Cour. After several months constant endeavor, I came to the conclusion that synchronism in the manner set forth was impossible, and in the light of subsequent experience that conclusion has been fully sustained and proven. In the summer of 1882, experimentation led me to think that synchronous telegraphy could be accomplished by using *two* line wires, one for vibrating the forks in unison, the other for telegraphic purposes. Upon *this* representation to the parties interested, the patent was bought from Mr. La Cour.

In the fall of 1882, Mr. La Cour submitted a plan for maintaining synchronism, in all respects the same as that set forth in his English Patent, October 7, 1882, of which he has sent you a copy. I have the original manuscript describing that plan. Mr. Calahan and myself carefully followed the instructions laid down, and although several days were spent in trying, we were unable to obtain synchronism for one minute. We had, by constant changing of adjustments, obtained accidentally approximate synchronism for a few minutes by the original plan of La Cour during a period of nearly six months, but failed utterly to obtain any encouragement from the new plan.

Mr. La Cour states that in June, 1880, he had constant synchronism between Fredericia and Nyborg, and that the Danish Director of Telegraphs has testified to the fact. This was nearly a year before the invention of his improved plan, which he himself states was invented in April, 1881.

Now, with all due respect to the civilities of life, I must simply state that such a thing was, is, and always will be absolutely impossible. If the INSTITUTE will examine La Cour's United States Patent 203,423, they cannot fail to find that synchronism under such conditions (and they are the same as

used by La Cour between Fredericia and Nyborg) is a physical impossibility. The conditions were, briefly, these:

At one station a fork made and broke automatically its own circuit through a local battery at a rate of about ninety times per second. This fork rotated a phonic wheel, the trailing arm of which made contact with numerous segments placed in the path of its rotation. With six of these segments, a main battery was connected. At the distant station was placed another fork and phonic wheel. This fork was not vibrated by a local battery, but by the six impulses which came over the line at each rotation of the trailer at the first station, numbering in all about thirteen per second, *provided the second phonic wheel was first in synchronism* with the first phonic wheel; otherwise the impulses coming over the line could not be delivered to the fork magnet. With these eighteen impulses over a line, the second fork was expected to vibrate in synchronism with the fork at the first station, which made for itself automatically ninety vibrations per second. Synchronism was first to be obtained by spinning the wheel by hand in order to get the fork at the second station to vibrate, and when once in vibration, this fork, with eighteen impulses per second, was to vibrate synchronously with the other, and without any correcting application whatever; and yet Mr. La Cour tells the FRANKLIN INSTITUTE that by this method he had for some time, incessantly from morning till night, synchronism between Fredericia and Nyborg, and that the managing Director of the Danish Government Telegraph has given a testimony to show that this synchronism works perfectly accurate and reliable, and was easy to manage.

Four years trial in America by competent and skilful electricians and inventors, including Mr. La Cour himself, failed to bring about in the slightest degree any such results. Even though the fork at the second station had been vibrated automatically by a local battery in the same manner as the fork at the first station, synchronism without correcting impulses would have been impossible. I have tried it thousands of times with instruments side by side.

The INSTITUTE can form its own opinion as to the correctness of Mr. La Cour's statement, that he had perfect synchronism "from morning till night."

Now, to return to Mr. La Cour's improved plan as communicated to me through Mr. Jones, and as exhibited by him (La Cour), in Denmark, Paris, Vienna, etc., and which he charges is substantially the same as used by me, I beg to state that this plan is not only not synchronous, but absolutely *anti-synchronous*, for, as the committee will find upon examination, and as I can prove by demonstration, should instruments be brought to synchronism, accidentally or coincidentally, the means provided by Mr. La Cour for regulating them in synchronism would drive them out of synchronism. Reference to his description and drawings will show that the trailer H at Station 2, is expected to touch *two* contacts p p, while the trailer at Station 1 is on one of the contacts o e. And no matter which position the armature of the polarized relay is in, the fork and wheel *must be going either too fast or too slow*. They are not allowed to remain synchronous. Again, supposing the armature T of the polarized relay is in the position shown in the drawing, the fork is then

vibrating too fast, a reverse current throws the armature over to its other stop to make it go slow, but while the armature is passing from the fast to the slow battery, the fork circuit being *entirely broken*, the rate of vibration of the fork is enormously increased, and the instruments driven away from even approximate synchronism. Therefore, when the distributor at Station 2 requires a retarding correction, it must first be accelerated far beyond the rate which called for retardation.

It must also be understood that the most delicate balance will not prevent the instruments from constant variation. If the change from the strong to the weaker battery is sufficient to *accelerate* the speed of the second instrument against its natural tendency to go slow at the time, this acceleration being *constant*, will also be sufficient to cause the instrument to gain sufficiently in one revolution, to entirely miss the segment for a retarding impulse on the next revolution, and *vice-versa*.

Assuming that a line, say of fifty miles or upwards, connects the two instruments, if a correcting impulse for slowing is needed, the trailer at Station 1 must be on battery segment *e* when the trailer at Station 2 is on the left hand contact *p*, then on account of retardation of the impulse in the line, the impulse will not arrive at Station 2 until the trailer at that station has passed off of the left-hand segment *p*, and no correction will be applied.

Again, even if it were admitted that all else worked as Mr. La Cour claims it will, it must be assumed that his correcting segments to which the batteries are connected, are as broad as the other segments used for telegraph signals (for otherwise the correcting impulses would not charge the line sufficiently to be felt at the distant end) then this being the case, it follows that the trailer at Station 1 must be, say, half way on the battery segment before the line can be charged sufficiently to change the position of the armature of the polarized relay, and, consequently, the variation of the trailers would be from the middle of one segment to the middle of the other, or equal to two segments, for the spaces between the segments must be included, and each space must be equal to the width of each segment to prevent interference between the segments forming the circuits. Therefore, the instruments would be out of synchronism two-thirds of the time. The charging point is indicated by

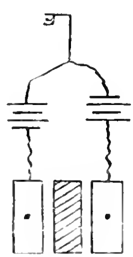


FIG. 3.

the dots on the segments. Mr. La Cour makes no provision for static discharge, "tailing," or retardation of the current. The quality of all his corrections must be the same; a slight variation cannot be rectified, nor is the

correction in any way proportioned to the discrepancy in synchronism. I do not deem it necessary to go into comparisons between Mr. La Cour's attempts and my synchronism, believing, as I do, that the FRANKLIN INSTITUTE fully appreciated the difference when it conferred its honors.

Mr. La Cour's phonic wheel patents have been abandoned in every country in Europe, and undoubtedly would have been forgotten in America, had it not been for my synchronism. His efforts for synchronism have been tried by various Government telegraph administrations without any success whatever. In a recent interview with Mr. La Cour, in Berlin, he admitted to me that he had no telegraph system, and in the presence of Herr Dr. Prof. Zetzsche, of the *Electro-Technische Zeitschrift*, he positively declined to discuss synchronism as a relative term, or the degree of synchronism claimed by him. I challenged his ability to obtain two circuits over a single wire, but he declined it. It is not necessary for me to remind the FRANKLIN INSTITUTE that synchronism at best is but a relative term; that it is all a question of degree. The Highes, Meyer and Baudot systems are all synchronous

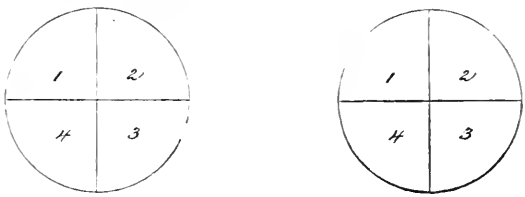


FIG. 4.

within certain limits. Two trailers, touching corresponding sectors of these circles, might be called synchronous. But synchronism on segments one millimetre in width, in a circle five inches in diameter, with trailers revolving at the rate of three times per second, is quite a different thing.

My system, which, for the past six months, has been in constant daily use on the English lines between London and Manchester, 200 miles, including nine miles of underground line, maintains synchronism within the 1,000th part of a second, for days at a time, without a second's interruption. On short lines, synchronism and octuplex telegraphy is entirely practicable—the variation never exceeding the 2,000th part of a second.

My system has been adopted by the English Government after exhaustive tests, and with full knowledge of Mr. La Cour's attempts and claims. Here, in Germany, I am now in negotiation with the Government for the use of my system. The La Cour system has been before them for more than seven years, and his instruments are among their shelved collection.

Mr. La Cour refers to the use of his system by Mr. Cassagnes, who is conducting experiments with steno-telegraphy between Paris and Marseilles. I know nothing of these experiments. I have received two inquiries by mail from Mr. Cassagnes, asking for terms under which my synchronism could be used by him, and I am credibly informed that in his prospectus Mr. Cassagnes bases his claims of advantage of his steno system upon the use of the Delany synchronism.

Mr. La Cour refers to pan-telegraphy incidentally, as if it was a trifling matter, easy of accomplishment by his system. But I have never heard o any results in this direction by Mr. La Cour, not even between Fredericia and Nyborg. The fact is, Mr. La Cour has not now, nor has he ever had any synchronism worthy of the name, and as for synchronous multiplex or fac-simile telegraphy, I don't think he knows anything about them. He has not, so far as I know, accomplished anything whatever in these directions.

It may be interesting to the INSTITUTE to know that I have recently obtained practically the most perfect synchronism by the system set forth in my patent 322,695, July 21, 1885, *i.e.*, bringing a yielding finger or spring attached to an electro-magnet armature in contact with the vibrator, so as to press against it. The pressure of the finger against the vibrator limits the amplitude of its vibration, and increases its rate. I have made a practical demonstration of it, not because my other methods were not good enough, but because the German Patent Office disputed my theory and required proofs. This system is entirely mechanical, so far as the reed is concerned.

Furthermore, I obtain perfect synchronism by an entire reversal of Mr. La Cour's theory, thus:

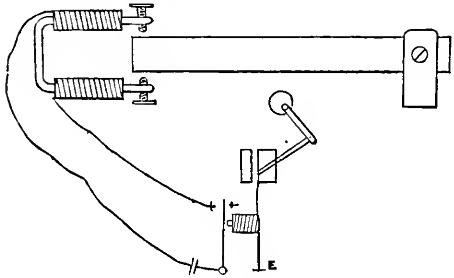


FIG. 5.

the charging of the correcting magnet, the poles of which are presented to the upper and lower edges of the vibrator *increases* the rate of vibration. The breaking of the circuit causes the lowering of the rate.

With my other independent correcting magnet the poles of which are projected towards the *sides* of the vibrator, thus:

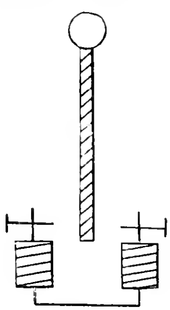


FIG. 6.

The greater the attraction of the correcting magnet the smaller the amplitude and the *lower* the rate of vibration. When the current is withdrawn the amplitude is increased and the *rate* also.

When the FRANKLIN INSTITUTE finds that I hold its high favors unworthily, I will be ready to surrender them, but the record of the INSTITUTE for fair and able investigation and discrimination leaves no apprehension in my mind on this head. Very respectfully,

PATRICK B. DELANY.

APPENDIX 3.

Direction
des Télégraphes d'Etat
de Danemark.

COPENHAGUE, le 9 Février, 1887.

Dear Sir:—In reply to your letter of January 24th, I have the honor to inform you that in the year 1880, during twenty-four days from the 17th June to the 10th July, arrangements were made on the telegraph offices of the Danish Government at Fredericia and Nyborg for sending two messages simultaneously over one wire by means of Morse apparatus and separate circuits produced by phonic wheels, invented by Poul La Cour, as synchronous instruments.

I regret that I have not seen the experiments myself, but I am in possession of a diary, held by an officer of the Danish Telegraph, whereby it is stated that during that time, from the 17th June to the 10th July, 1880, the phonic wheels in Fredericia and Nyborg were working every day from 8 or 9 in the morning to 9 in the afternoon, only with a few short interruptions caused by several events, such as disturbances on the telegraph wire or mechanical disturbances on the phonic wheels. For the period from the 22d June to the 10th July, it is expressly remarked that the synchronism was good.

I am further in possession of official reports, dated 28th June, 1880, from the head officers of the telegraph offices at Fredericia and Nyborg, after which the apparatuses on the both offices have been working regularly, and messages have been sent since the 17th June, except two days, when the working of the phonic wheels was disturbed by mechanical causes. In these reports it is especially stated that on the 27th June, 1880, were sent *over one wire*, from 9 hours, 57 minutes to 10 hours, 19 minutes simultaneously, four messages on each of the two circuits from Fredericia to Nyborg, eighty words on the one and seventy-seven words on the other circuit.

From 10 hours, 20 minutes to 10 hours, 36 minutes simultaneously, four messages on each of the two circuits from Nyborg to Fredericia, sixty-eight words on the one circuit, and sixty-three words on the other.

From 10 hours, 48 minutes to 11 hours, 2 minutes simultaneously, four messages (sixty-eight words) from Nyborg to Fredericia and four messages (sixty-five words) from Fredericia to Nyborg.

These messages, as well as the paper slips, such as received at the office of Fredericia, are in my hands.

After this, there is no doubt that in the year 1880 in this country, messages have been sent simultaneously over one wire by means of separate circuits produced by the phonic weeels, invented by Mr. Poul La Cour. Before that time there was not, nor has there ever since been any telegraph line in this country operated by synchronism as a multiplex system.

With great respect I remain,

[SIGNED] Very truly yours,

Director of the Danish Government Telegraphs.

MR. E. ALEX. SCOTT, Chairman of Committee of the FRANKLIN INSTITUTE, Philadelphia.

APPENDIX 4.

Ministere
des Postes et des Télégraphes.

Direction
du Materiel de la Construction.

2 Bureau. A.

Objet.

PARIS, le 26 Février, 1887.

Monsieur le Président:—Par lettre du 24 Janvier, vous voulez bien me demander si, avant 1881, on a réussi en France à augmenter la capacité de trafic des fils télégraphiques par l'usage d'appareils synchroniques à transmission multiple.

Je m'empresse de vous donner à cet égard les renseignements suivants:

(1°.) Dès 1858, Monsieur Rouvier, actuellement Directeur des Postes et des Télégraphes en retraite à Nîmes (Département du Gard), a imaginé un système de transmission multiple, comportant l'emploi de deux pendules rendus synchrones, et l'intercalation, entre les signaux d'une lettre Morse transmise par un poste, des signaux de lettres transmises par d'autres postes. Une description de ce système, faite par l'auteur a été insérée dans le recueil des *Annales Télégraphiques* de l'année 1860 (livraison de Janvier et Février, page 5): cette description a été rappelée et résumée dans un article sur la transmission multiple inséré dans les *Annales Télégraphiques* de l'année 1874 (livraison de Septembre–Octobre, page 187).

(2°.) En 1871, M. Meyer, agent de l'Administration francaise, aujourd'hui décéde, a réalisé un système de *transmission multiple*, fondé sur l'emploi de deux distributeurs circulaires parcourus par deux aiguilles synchrones, et sur l'intercalation, entre les lettres Morse transmises par un poste, des lettres transmises par d'autres postes. L'idée de la division du temps par des distributeurs circulaires synchrones avait été empruntée par M. Meyer à l'appareil typo-télégraphique de MM. Vairn et Fribourg, imaginé en 1864 et décrit dans le traité de télégraphie électrique de M. Rouvier de 1867 (Tome II, par. 839, pages 307 et 308).

Dans le système Meyer, l'impression des signaux Morse à l'arrivée s'effectuait à l'aide d'une hélice, disposée comme dans l'appareil autographique du même inventeur, appareil qui avait figuré à l'Exposition Universelle de 1867.

L'article des *Annales Télégraphiques* de Septembre–Octobre, 1874, déjà cité renferme également la description de l'appareil multiple Meyer. La livraison de Juillet–Août, 1876, du même recueil fait connaitre les perfectionnements apportés ultérieurement aux organes du système. Des appareils quadruples et sextuples qui functionnaient en service courant, sur un certain nombre de lignes françaises jusqu' à ces dernières années. Ils ont figuré à l'Exposition Universelle de 1876.

A l'Exposition d'Electricité de 1881, on remarquait encore un appareil de M. Meyer applicable au service simultané de plusieurs villes par un seul conducteur, un appareil multiple du même à récepteurs indépendants, et un appareil multiple de M. Willot à récepteurs Morse.

(3°.) L'appareil multiple imprimant de M. Baudot, aujourd'hui en usage sur toutes les lignes françaises à grand trafic, brevet le 17 Juin, 1874, a figuré aux Expositions de 1878 et de 1881.

Agréez, Monsieur le Président, l'assurance de ma considération très-distinguée.

Le Ministere des Postes et des Télégraphes.

[TRANSLATION.]

Ministry
of
Postal Affairs and Telegraphs.

PARIS, 26th February, 1887.

Mr. Chairman:—In your letter of the 24th January, you ask me, if, prior to the year 1881, we have succeeded in France in increasing the working capacity of telegraph lines, by the use of synchronic apparatus for multiplex transmission.

I hasten to give you the following information on this point:

(1.) About the year 1858, M. Rouvier, now Director of Posts and Telegraphs, stationed at Nîmes (Department of Gard), conceived a system of multiplex transmission permitting the employment of two synchronized pendulums, and the interpolation between the signals of a Morse letter sent by one station, the signals of letters transmitted by other stations. A description of this system, made by the inventor, was published in the issue of the *Annales Télégraphiques* for 1860 (January and February, p. 85).

This description was revised and condensed in an article on "Multiplex Transmission," in the *Annales Télégraphiques* for 1874 (September and October, p. 187).

(2.) In 1871, M. Meyer, Government agent, since deceased, produced a system of multiplex transmission based on the employment of two circular distributors traversed by two synchronized hands (pointers?), and on the interpolation between the Morse letters transmitted by one station, letters transmitted by other stations.

The idea of the division of time by circular synchronous distributors, had

been borrowed by M. Meyer from the typo-telegraphic apparatus of Messrs. Vairn and Freibourg, invented in 1864, and described in the treatise on electro-telegraphy of M. Blavier, of 1867 (vol. ii, sec. 839, and pp. 307–308).

In the Meyer system, the impression of the Morse signals at the receiving station was accomplished with the aid of a helix, arranged as in the autographic apparatus of the same inventor—an apparatus which figured at the Universal Exposition of 1867.

The article in the *Annales Télégraphiques*, of September–October, 1874, already cited, also contains the description of the multiple apparatus of Meyer. The July–August number (1876) of the same periodical, described the final improvements applied to parts of the system. Quadruplex and sextuplex apparatus have been in regular service on a certain number of French lines up to the last few years. They figured at the Universal Exposition of 1878.

At the Electrical Exhibition of 1881, we noticed another apparatus of M. Meyer, applicable to the simultaneous service of several cities by a single conductor (live wire?), a multiple apparatus of the same, with independent receivers, and a multiple apparatus of M. Willot, with Morse receivers.

(3.) The multiplex printing apparatus of M. Baudot, now in use on all the important French lines, patented 17th June, 1874, figured at the Expositions of 1878 and 1881.

Accept, Mr. Chairman, the assurance of my most distinguished consideration.

GRANET,

[SIGNED] *The Minister of Postal Affairs and Telegraphs.*

APPENDIX 5.

Kaiserlich Deutsches Reichs-Postamt
II. Abtheilung.

BERLIN, W., 11 Februar, 1887.

Ew. Wohlgeboren theilt das Reichs-Postamt auf das gefällige Schreiben vom 24. Januar ergebenst mit, dass in der deutschen Reichs-Telegraphenverwaltung bereits vor dem Jahre 1881 und auch bis in die neueste Zeit hinein verschiedene Methoden des Gegensprechens und der Multiplex-Telegraphie, und zwar mit befriedigendem Erfolge, Verwendung gefunden haben.

(SIGNED) HAKE.

An HERRN E. ALEX. SCOTT, Vorsitzender des Committee of the FRANKLIN INSTITUTE, Philadelphia.

II. 1594.

[TRANSLATION.]

Imperial German Postal Department.
II. Division.

BERLIN, W. 11th February, 1887.

In reply to your letter of 24th January, the Imperial Post Office Department has the honor to state that various methods of simultaneous transmission (*Gegensprechen*) and of multiplex telegraphy have been employed in the

Imperial German Telegraph Service from a period prior to the year 1881, down to the present time, and with satisfactory results.

[SIGNED] HAKE.

To MR. E. ALEX. SCOTT, Chairman Committee of the FRANKLIN INSTITUTE, Philadelphia.

II. 1594.

APPENDIX 6.

To the FRANKLIN INSTITUTE for the Promotion of the Mechanic Arts, Philadelphia.

On the 7th of July, 1886, I had the honor of sending to the most honorable INSTITUTE a communication concerning my priority to the synchronism, that Mr. Delany had mentioned to the INSTITUTE as his own. As I have been informed of Mr. Delany's asserting to certain gentlemen in Europe, that my synchronism could not move at all, and that its moving had not been effected until done by means of his additions, I suppose that he is speaking in a similar manner to the FRANKLIN INSTITUTE. I therefore take the liberty of forwarding here enclosed a testimony of the perfectly working synchronism, for which I took out a patent in France on the 11th of October, 1882, the very same synchronism for which I took out a patent in England on the 9th of October, 1882.*

The paper is notified by the respective authorities at France.

I should value highly if the most honorable INSTITUTE would embody this paper, together with my former communication in the JOURNAL OF THE FRANKLIN INSTITUTE. I remain, yours very respectfully,

[SIGNED.] POUL LA COUR.

Askovhus, Vejen Station, Denmark, the 12th March, 1887.

A.

Ministere
des Postes et Télégraphes.

PARIS, le 4 Janvier, 1887.

Monsieur Cassagnes, Ingénieur civil, 18 Rue La Fayette à Paris.

Monsieur:—Conformément au désir exprimé dans votre lettre du 1 Décembre, je vous envoie ci—joint un extrait des rapports adressés à l'Administration, à la suite des expériences en ligne de votre système de Sténo-télégraphie.

Récévez, monsieur, l'assurance de ma considération distinguée.

GRANET.

Le Ministre des Postes et des Télégraphes.

Extrait—du rapport sur les expériences de transmission télégraphique faites au moyen du système Steno-télégraphique Cassagnes.

Dernière Série d'Expériences (*Octobre, 1886*).

La dernière série d'essais de steno-télégraphie multiple, faite en utilisant

* Letter and translation appended, and marked A.

la composition préalable et la transmission automatique, a donné les résultats suivants.

On peut transmettre par minute et par secteur de 125 à 145 mots; quant au nombre de transmissions, ou secteurs, il dépend de la résistance et de la capacité dè la ligne. Un appareil triple, ou à 3 secteurs, pourrait être utilisé jusqu' á 500 ou 600 kilomètres, un appareil double jusqu' á 700 ou 800, ou peut être au delà; mais dans les essais qui ont été faits sur un circuit de 900 kilomètres, la correction n'était pas toujours efficace et il en résultait des interférences nuisibles. Le rendement par heure serait donc de 25,000 mots environ entre Paris et Lyon, et de 15,000 à 16,000 sur de plus longues lignes, toutes réserves faites en ce qui concerne le langage sténographique qui forme la base du système.

Je certifie pour copie conforme l'extrait du rapport de l'Aministration française des Télégraphes qui précède.

Je certifie de plus que dans mes appareils sténo-télégraphiques dont les résultats *en ligne* sont officiellement constatés ci-dessus, le seul synchronisme dont j'ai fait usage—et qui n' a cessé de fonctionner de la manière la plus satisfaisante sur des circuits français de (800) huit cents kilomètres—est le synchronisme par la roue phonique de Monsieur Poul La Cour d'Askovhus (Danemark) tel qu'il été breveté en France par Monsieur La Cour le 11 Octobre, 1882.

[SIGNED] A. CASSAGNES.

Paris, le 2 Mars, 1887.

A

[TRANSLATION.]

Ministry of Postal Affairs and Telegraphs.

PARIS, the 4th January, 1887.

Monsieur Cassagnes, Civil Engineer, 18 Rue LaFayette, Paris.

Sir:—Conformably to the desire expressed in your letter of the 1st December, I send you annexed an extract from the reports addressed to the Government, in consequence of the experiments on the line of your system of steno-telegraphy.

Receive, sir, the assurance of my distinguished consideration.

GRANET.

The Minister of Postal Affairs and Telegraphs.

Extract from the reports on the experiments in telegraphic transmission, made by means of M. Cassagnes's steno-telegraphic system.

Last Series of Experiments, October, 1886.

The last series of experiments in multiplex steno-telegraphy, made by utilizing the previous composition, and automatic transmission, has furnished the following results:

We can transmit per minute and per section from 125 to 145 words; as for the number of transmissions or sections, it depends on the resistance and the capacity of the line. A triplex apparatus or of three sections, might be utilized up to 500 to 600 kilometres (310,675 miles; 372,810 miles), a duplex apparatus up to 700 or 800 kilometres (434,945 miles; 497,080 miles), or perhaps beyond; but in the experiments made on a circuit of 900 kilometres (559,215 miles), the correction was not always efficacious, and injurious interferences resulted. The accomplishment per hour would then be about 25,000 words between Paris and Lyons, and from 15,000 to 16,000 on longer lines, every reservation being made as to what concerns the stenographic language forming the basis of the system.

I attest, as a certified copy, the preceding extract from the report of the French telegraph administration.

I certify, moreover, that in my steno-telegraphic apparatus, the results of which on the line are officially stated above, the only synchronism of which I have made use, and which has not failed to operate in the most satisfactory manner on French circuits of 800 kilometres, is the synchronism by means of the phonic wheel of Monsieur Poul La Cour of Askovhus (Denmark) as patented in France by M. La Cour, the 11th October, 1882.

A. CASSAGNES.

Paris, 2d March, 1887.

March 28, 1887.

To Mr. POUL LA COUR, Askovhus, Vejen Station, Denmark.

Dear Sir:—Acknowledging the receipt of your communication of March 12, '87, with enclosure from the Ministere des Postes et Télégraphes, of Paris, I have the honor to inform you that this communication, as well as your earlier one, of July 7, '86, has been referred to a special committee for proper investigation. The committee has given the subject careful examination, and the report thereon is expected to be ready for presentation at the next stated meeting of the INSTITUTE (April 20, '87).

The whole subject will undoubtedly appear in the JOURNAL as soon as the Committee's Report shall have been formally acted on by the INSTITUTE.

I have the honor to remain your obedient servant,

[SIGNED] WM. H. WAHL, *Secretary*.

STUDIES ON THE STRATIFICATION OF THE ANTHRACITE MEASURES OF PENNSYLVANIA.

BY HENRY A. WASMUTH, Instructor of Mining, University of Pennsylvania.

The State Geological Survey advocates the theory, that the anthracite measures have been folded into numerous "inversions," without fracture of the strata.

The general theories of "faults," established by long experience in mining in Europe, are described and advocated in the *Report of the First Geological Survey of Pennsylvania.*

On the strength of the general theories of faults above mentioned, and my long experience in studying and developing faults, varying much in character and extension, I maintain:

That in "bedded" mineral deposits no "inversion" or "overlapping" of the strata can take place, without fracture and more or less dislocation: and that, in general, the dislocations of the strata take place in one of two ways; either the portion of a mineral deposit on the hanging wall of the fracture or fault is in a lower position, than the portion on the foot wall, as illustrated in Fig. 1, or it is in a higher position, as illustrated in Fig. 2. Occurrences, as illustrated in Fig. 1, are called "transverse faults;" occurrences, as illustrated in Fig. 2, are called "longitudinal faults," or overlaps.

In order to explain this matter conveniently, I will first quote some extracts from the *Reports of the First and Second Geological Survey of Pennsylvania*, and then illustrate and describe a number of faults developed in anthracite mining, and apply the facts, established thereby, in the examination of some of the sections of the *Second Geological Survey of Pennsylvania.*

EXCERPTS.

The researches of the Geological Survey, and the experience, often very dearly purchased, of the conductors of our anthracite mines, have at least induced a very general conviction of the necessity of attending to all the anticlinal and synclinal turns of the strata, for it is now admitted that these are the true key to the opening, tracing and successful working of the coal seams of the region, etc.*

* *First Geol. Survey of Penn, ii,* 28.

Contortions of the coal beds unaccompanied by fracture, are very numerous in some portions of the coal field. Where the flexure reaches its maximum degree, the short leg of the anticlinal becomes of course inverted, and then the coal, in all this part of the bed and at both the anticlinal and the synclinal elbows, is in a compressed and fragmentary state. There are other irregularities of the coal arising from a sudden change of inclination, once or oftener, within a small space. Such usually announce proximity to a crushed axis, or, what is the same thing, to a longitudinal dislocation, which is but the extreme limit of the displacement, that, towards its termination closes, and becomes a mere fold or axis.*

The actual displacements or breaches of contact in the coal may be divided into two classes; one, embraces every form of disturbance in the coal bed parallel to its dip; the other, every variety of cross fracture. The first might, for convenience, be called *parallel slips*, the others are named by their usual title of *faults* or *cross fracture*. In very many instances, the parallel slips present an extensive disturbance of the mass of the coal bed within itself, without more displacement of the roof and floor than amounts to a series of local bulgings and contractions of the layer. *The coal has rubbed upon itself, and is in a more or less fragmentary, friable and polished state.* To admit of this parallel displacement or moving forward of one part of the bed upon the other, there must have arisen somewhere, it is obvious, *either a fracture or a doubling upon itself of the roof, or the floor, or of both.* It has been already shown that the sharp mountain beds illustrate to an extraordinary degree this form of displacement by parallel slipping.

Not infrequently the miner comes upon fractures in the roof or the floor of his coal bed, which explain to him the origin of local parallel dips. In this cases, the one boundary of the bed giving way, and not the other, the forward movement of the coal causes one of the broken portions of the roof or floor *to pass by or overlap* the other, and even to bend and curl up under the prodigious pressure applied to it. This is exemplified in the accompanying section of a disturbance of this nature in the great bed of Lorberry Creek, where the floor has broken, become involved in the sliding coal, and curved back upon itself until it has met the roof. At

* *First Geol. Survey of Penn., ii*, 236.

this point the bed, the proper thickness of which is about twenty-seven feet, has been swollen by the overlapping of its splintered ends to a size of fifty feet. *Fig. 2A.*

Of the other class of displacements, *the cross fractures or true faults,* little need be said here in detail. *Where they range with the strike* or course of the strata, they will be found *to terminate generally in anticlinal folds or flexures, first closely compressed and crushed, but farther on becoming more regular,* and at last dying out into the general dip. *Such longitudinal faults bring a rock wall against the lower end of the coal bed,* and in each instance where a *valuable vein* is thus abruptly cut off, it becomes a problem of much practical interest to determine the direction in which the last portion of the coal bed is to be sought.*

It may be accepted as a general rule, not without its occasional exceptions, that these longitudinal faults consist of *an upthrow of the vein to the south of the fault,* so that the remainder of the vein, if we are mining downward, will be recovered at a point above and further back towards the outcrop, compared with the spot where the rock wall crosses the mine.

Tracing such a fault through its successive stages, we shall generally be able to resolve it into a broken, oblique flexure, and then in a gentle, normal one. The three conditions of the dislocated flexure are illustrated in the diagrams here annexed, *Fig. 2B.* Another species of cross fracture or dislocation is where *the break is transverse to the strike or course of the strata,* sometimes perpendicular to it, sometimes oblique. These displacements bring a rock wall abruptly against the vein in the direction of its length; or, in other words, at the end of a gangway or level, and only *where they are oblique do we encounter them also in the direction of the dip.*

When traced for any considerable distance across the strata, such a fault will be found to be connected with a difference in the slope of the bed upon its two sides, the one portion having been more uplifted than the other, and therefore shoved forward beyond it. These are all instances to be explained by some inequality in the transverse pressure, propagated northward or southward to the dislocated strata; and this inequality will usually be found to

* *First Geol. Survey of Penn.*, ii, 237.

proceed either from the sudden rising of an anticlinal flexure to the north or south of the position of the fracture, or from a sudden termination of a longitudinal dislocation.

Where an obliquely transverse fault divides the strata, we shall usually discover that the most uplifted set of rocks has been shifted forward beyond the termination of the other less inclined portion.

By this means, the shapes of the rolls and overturns are exhibited, and means are afforded to mining engineers and superintendents for directing their headings with less anxiety respecting what is in advance of them.

The method of constructing cross sections, by Mr. Ashburner, will be closely criticised by the mining engineers of the region. I am convinced that they will not only respect the conscientious carefulness, but more and more admire the scientific skill with which this part of the work has been done. I think that they cannot help feeling a considerable confidence in the probality of the construction wherever there is a lack of actual data. At all events, many new and useful ideas will be contributed to the engineering science of the region.

During the latest mining excursion, the following-named collieries have been visited:

Kohinoor	Colliery, controlled by the	P. and R. C. and I. Company.	
Middle Creek	"	"	"
Otto	"	"	"
East Franklin	"	"	"
West Brookside	"	"	"
Williamstown	"	"	Penna. R. R. Company.

Kohinoor Colliery.—The Mammoth and Holmes beds have been opened by two separate shafts within a short distance. The Holmes bed dipping about 12° south, has been cut off by a longitudinal fault in the west gangway. For the recovery of the lost portion of the bed, a cross cut towards the hanging wall was commenced as illustrated in *Fig. 3*. The rock through which the cross cut passed is parallel to the Holmes bed for a distance of about twelve to fifteen feet and then a distinct fracture or fault, dipping about 45° south, has been intersected. The width of the fault is about two inches, and it is filled with disintegrated rock and coal.

Fig. 10.

Approx. Groundplan of East Franklin Colliery.

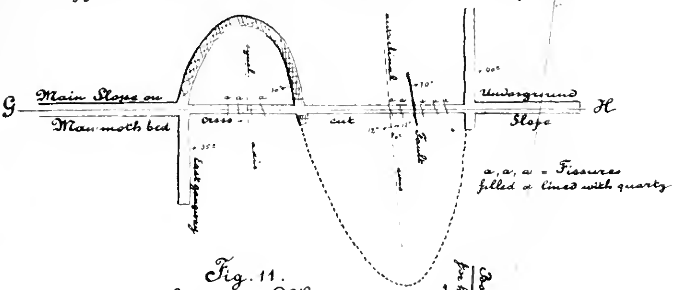


Fig. 11.

Section GH.

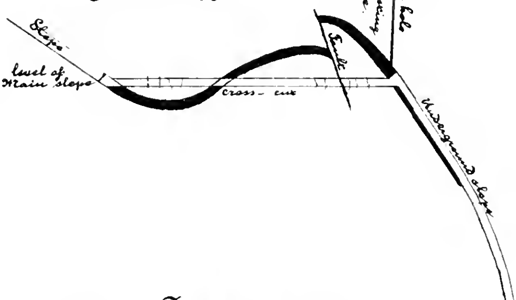


Fig. 12.

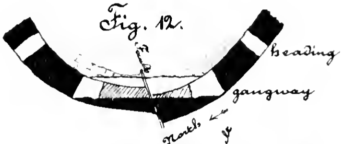


Fig. 13

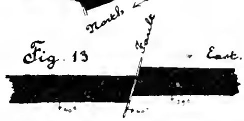


Fig. 14.

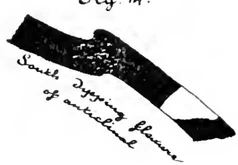


Fig. 16.
x. Section of
mstown Tunnel.
Scale 300's 1"

Fig. 32.

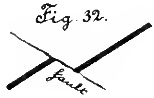


Fig. 31.

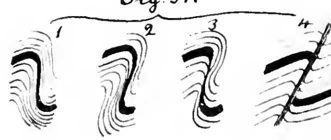
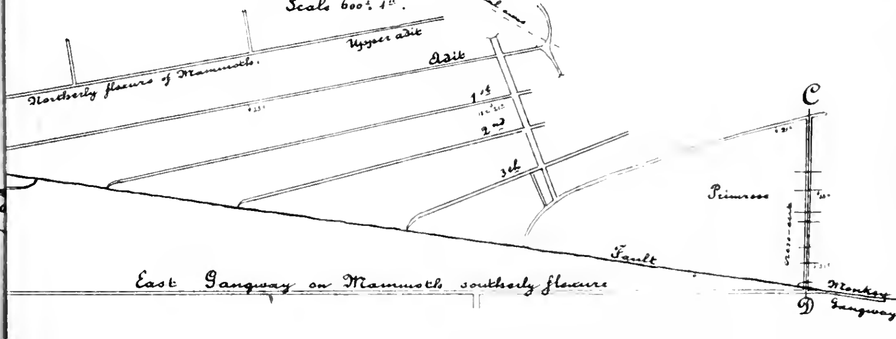


Fig. 7.

Approximate Groundplan of Otto Colliery.

Scale 600' 1".



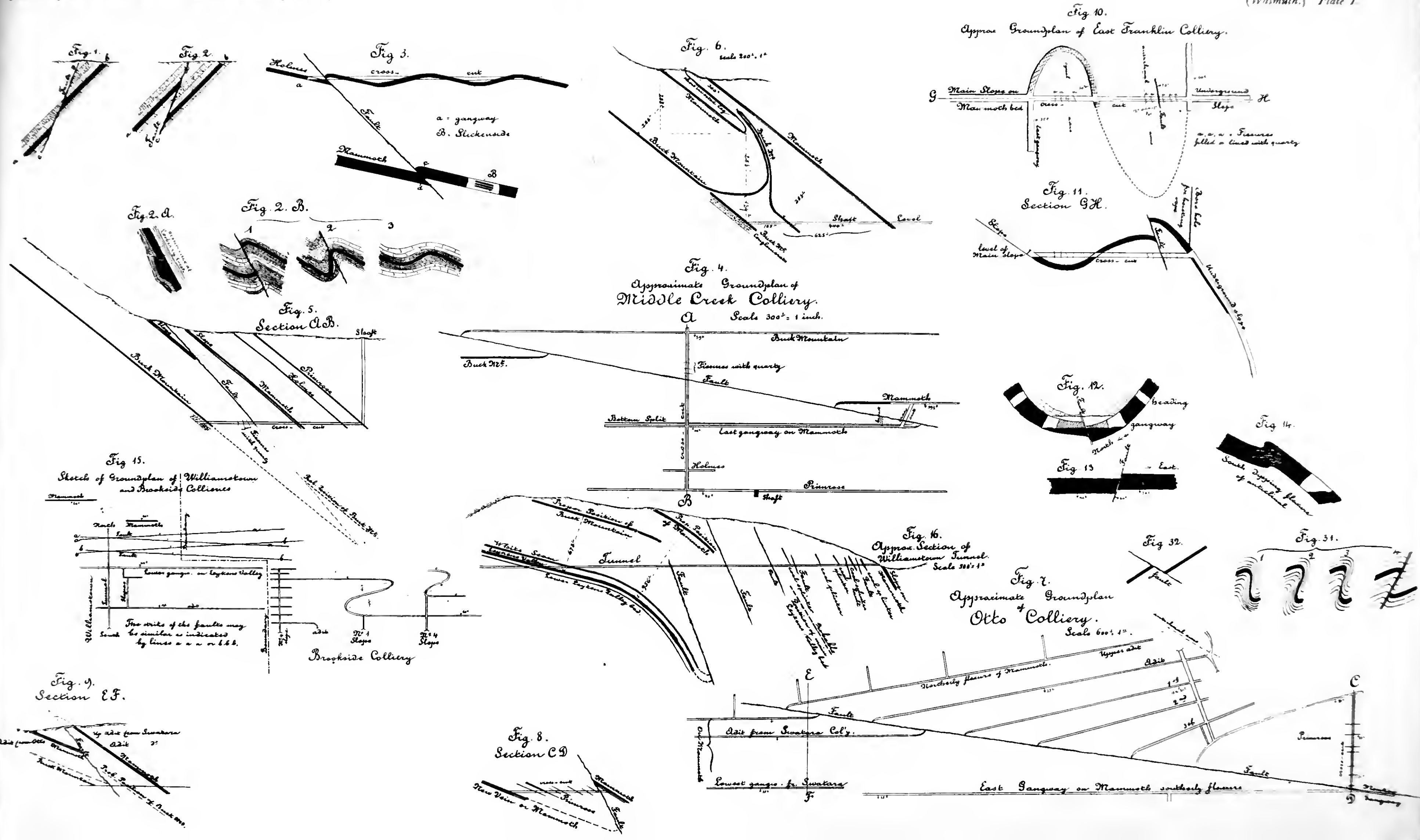
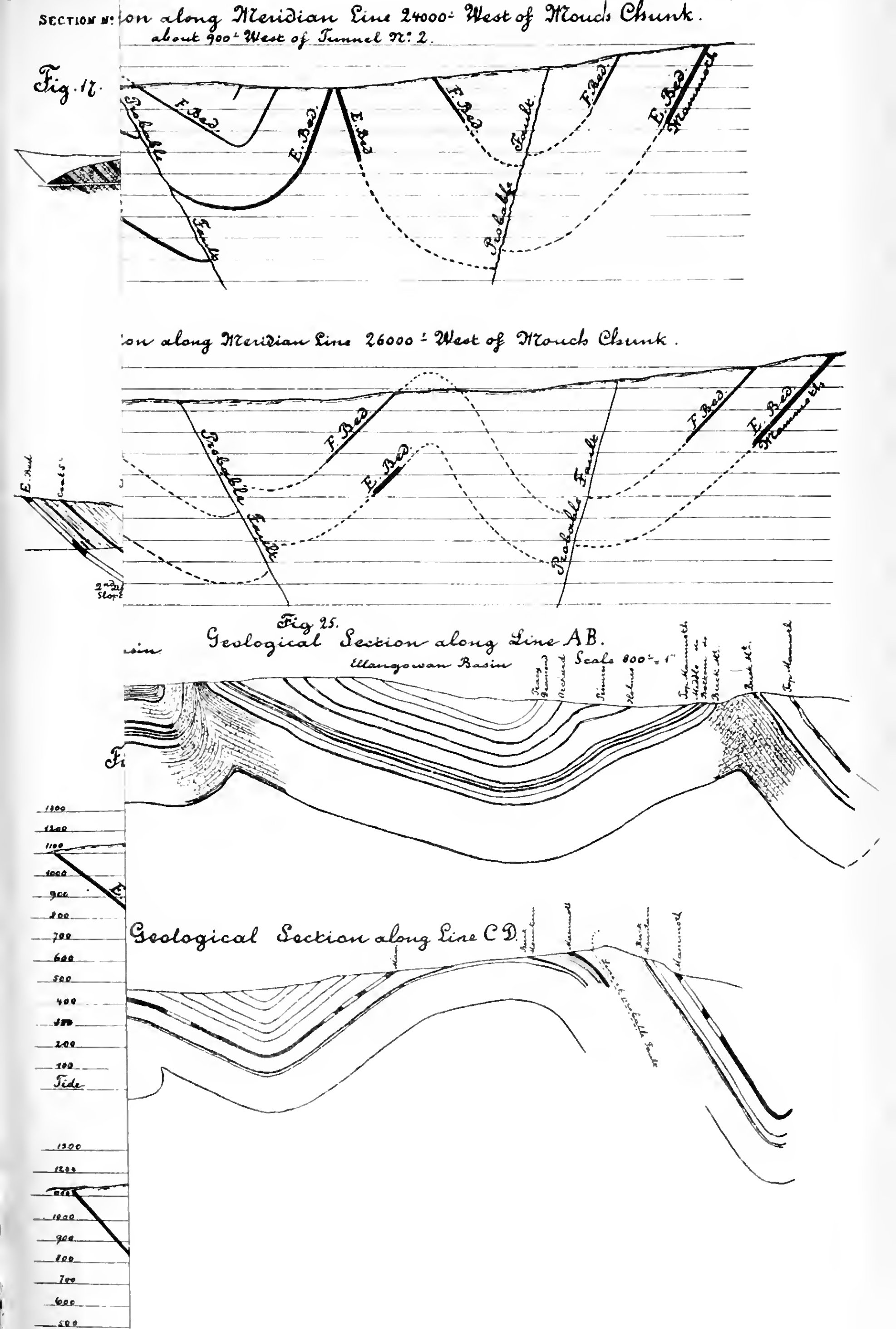
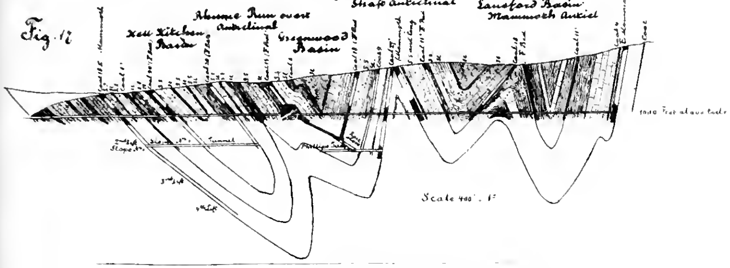


Fig. 1.
Fig. 2.
Fig. 3.
Holmes
cross-cut
a = gangway
B. Slickenside
Mammoth
Fault
Fig. 2. A.
Fig. 2. B.
Fig. 6.
Buck Mountain
Mammoth
Shaft
Level
Fig. 10.
Approx. Groundplan of East Franklin Colliery.
Main Slope on Mammoth bed
Underground Slope
a. a. a. Fissures filled & lined with quartz
Fig. 11.
Section GH.
level of Main slope
Underground slope
Fig. 4.
Approximate Groundplan of Middle Creek Colliery.
Scale 300 ft = 1 inch.
Buck Mountain
Buck Mt.
Fissures with quartz
Fault
Mammoth
Bottom Split
East gangway on Mammoth
Holmes
Primrose
Shaft
Fig. 5.
Section AB.
Shaft
Buck Mountain
Fault
Mammoth
Holmes
Primrose
Fig. 12.
heading
gangway
Fig. 13
East.
Fig. 14.
South dipping flanks of anticlinal
Fig. 15.
Sketch of Groundplan of Williamstown and Brookside Collieries
North
Mammoth
Fault
Williamstown
South
The strike of the faults may be similar as indicated by lines a a a or b b b.
Brookside Colliery
No. 1 Slope
No. 4 Slope
Fig. 16.
Approx. Section of Williamstown Tunnel.
Scale 300 ft = 1 in.
Proper Position of Buck Mountain
Tunnel
Fault
Fig. 32.
Fault
Fig. 31.
Fig. 7.
Approximate Groundplan of Otto Colliery.
Scale 600 ft = 1 in.
Upper adit
Adit
Northerly flexure of Mammoth
Fault
Adit from Swatara Col'y.
Lowest gangw. fr. Swatara
East Gangway on Mammoth southerly flexure
Primrose
Fig. 9.
Section EF.
Adit
Mammoth
Fig. 8.
Section CD
New Vein or Mammoth
Primrose
Mammoth
Fault


SECTION N: on along Meridian Line 24000: West of Mouch Chunk.
about 900: West of Tunnel N: 2.
Fig. 17.
F. Bed
E. Bed
Probable Fault
Mammoth
on along Meridian Line 26000: West of Mouch Chunk.
F. Bed
E. Bed
Probable Fault
Mammoth
Fig 25.
Geological Section along Line AB.
Ellangowan Basin
Scale 800: = 1"
Geological Section along Line CD.
Tide
100
200
400
500
600
700
800
900
1000
1100
1200
1300

SECTION No. 2 FROM NESQUEHONING VALLEY TO MAUCH CHUNK VALLEY THROUGH RHUME RUN (No. 1) TUNNEL.

Fig. 17



SECTION No. 3 THROUGH TUNNEL No. 2.

Fig. 18.

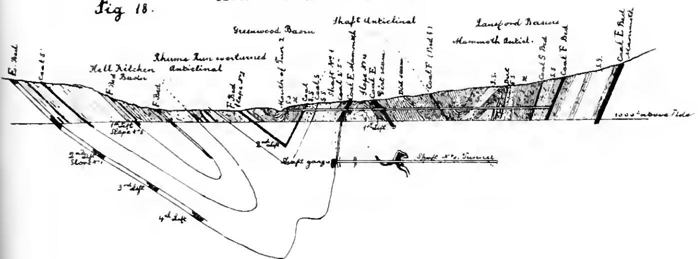


Fig. 19. Section along Meridian Line 20000' West of Mauch Chunk about 1600' East of Rhume Run Tunnel. (Vol. I. See Geol. Surv.)

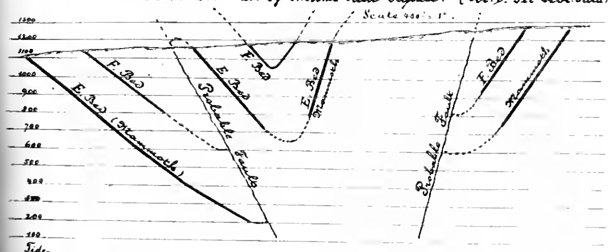


Fig. 20. Section along Meridian Line 22000' West of Mauch Chunk about 400' West of Rhume Run Tunnel.

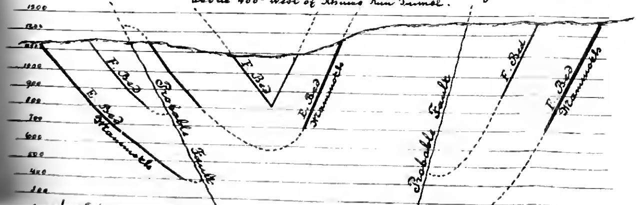


Fig. 21. Section along Meridian Line 24000' West of Mauch Chunk. about 900' West of Tunnel No. 2

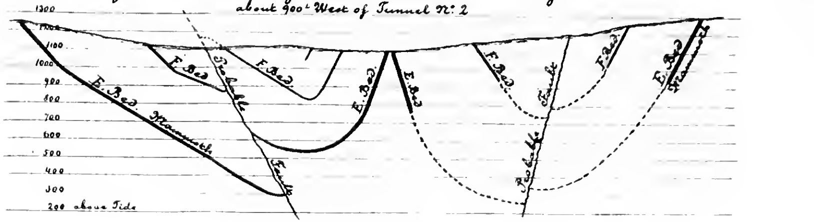


Fig. 22. Section along Meridian Line 26000' West of Mauch Chunk.

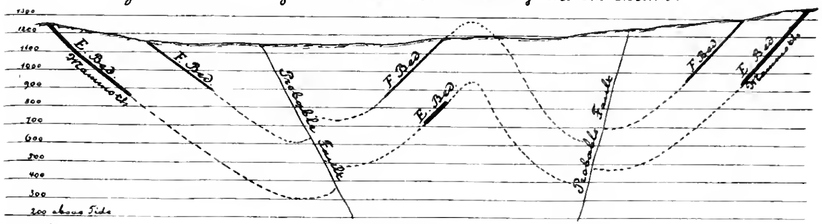
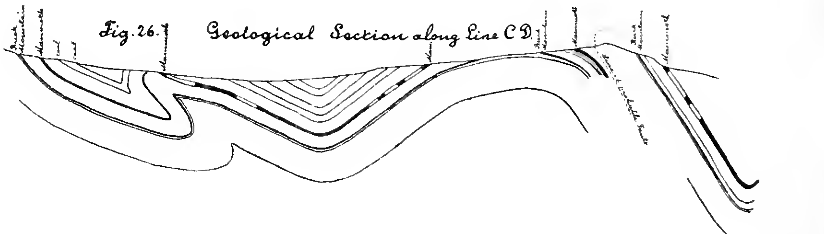


Fig. 25. Geological Section along Line AB. Scale 800' = 1"

Fig. 26. Geological Section along Line CD.



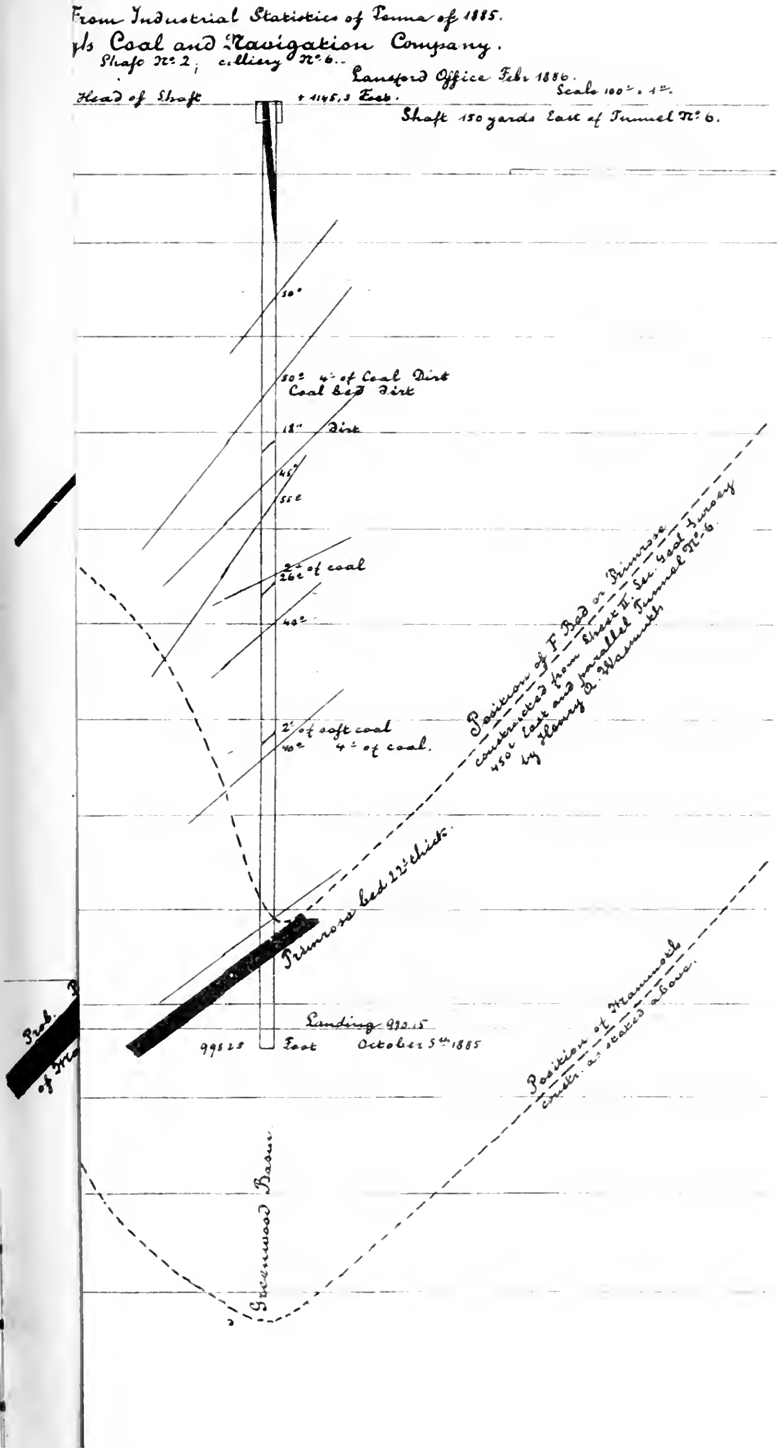

From Industrial Statistics of Penna of 1885.
Coal and Navigation Company.
Shaft No. 2; colliery No. 6.
Lansford Office Feb. 1886.
Scale 100 ft = 1 in.
Head of Shaft
+ 1145.3 Feet.
Shaft 150 yards East of Tunnel No. 6.
50°
50° 4' of Coal Dirt
Coal bed Dirt
18" Dirt
45°
55°
2' of coal
26°
40°
2' of soft coal
40° 4' of coal.
Position of F Bed or Primrose constructed from Sheet II. Sec. Geol. Survey 450' East and parallel Tunnel No. 6 by Henry D. Wasmuth
Primrose bed 22' thick.
Landing 992.15
995.28 Feet October 5th 1885
Position of Mammoth under as stated above.
Greenwood Basin
Prob.

Fig. 23.

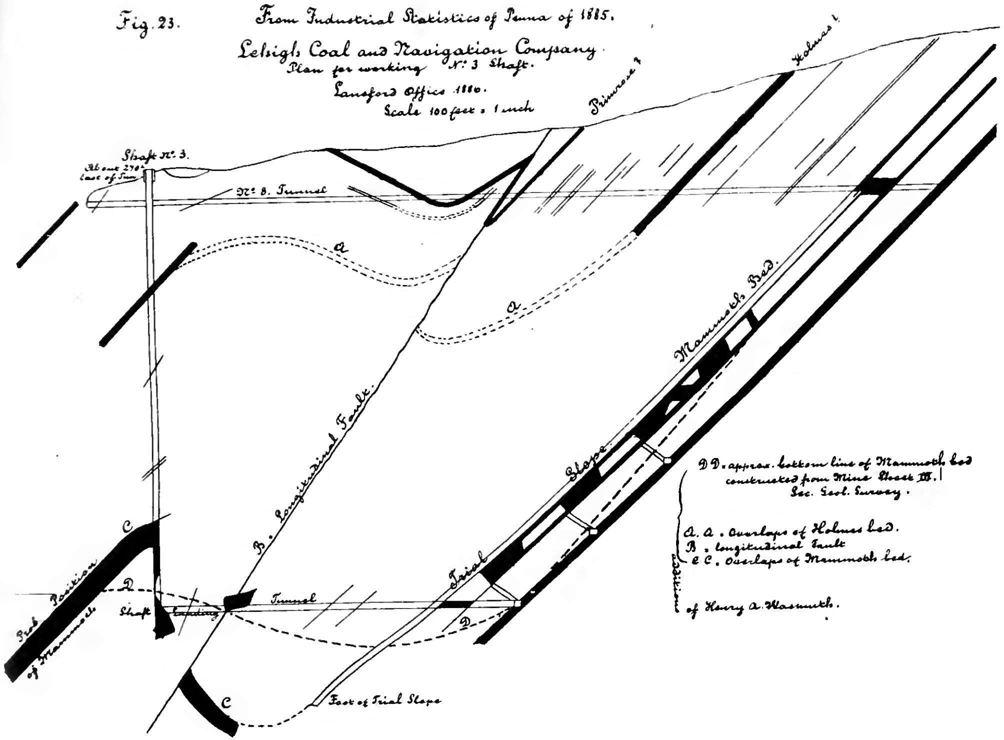
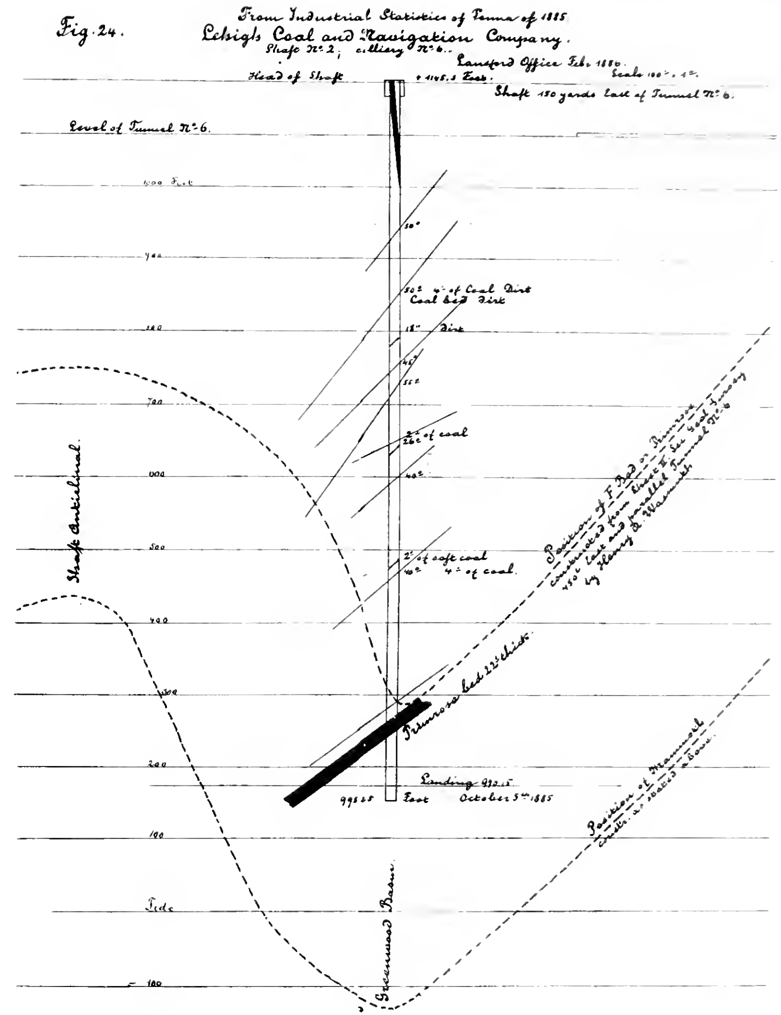
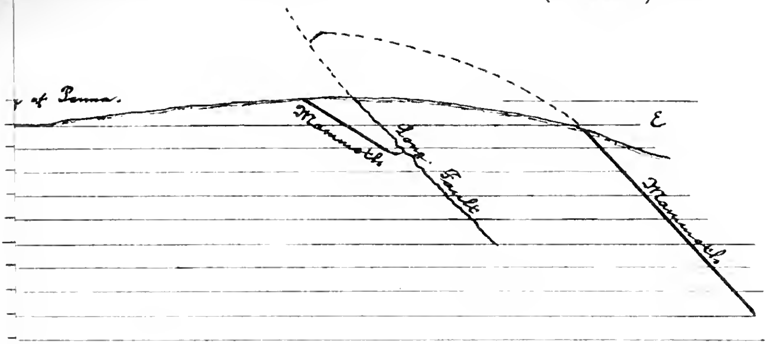
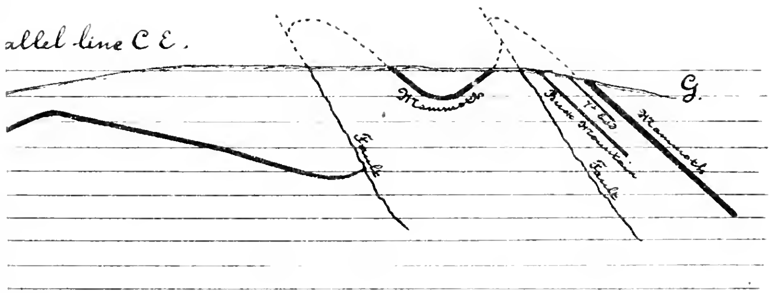

From Industrial Statistics of Penna of 1885.
Lehigh Coal and Navigation Company
Plan for working No. 3 Shaft.
Lansford Offices 1880.
Scale 100 feet = 1 inch
Shaft No. 3.
No. 8. Tunnel
Primrose ?
Holmes ?
A
Mammoth Bed.
B. Longitudinal Fault.
Slope
Trial
C
D
Shaft
Landing
Tunnel
Foot of Trial Slope
Position of Mammoth
D D. approx. bottom line of Mammoth bed constructed from Mine Sheet III. 2nd Geol. Survey.
A. A. Overlaps of Holmes bed.
B. Longitudinal Fault
C C. Overlaps of Mammoth bed.
Additions of Henry A. Wasmuth.

Fig. 24.


From Industrial Statistics of Penna of 1885
Lehigh Coal and Navigation Company.
Shaft No. 2; colliery No. 6.
Lansford Office Feb. 1880.
Scale 100 ft = 1 in.
Head of Shaft
Shaft 150 yards East of Tunnel No. 6.
Level of Tunnel No. 6.
Shaft Anticlinal
2' of soft coal
4' of coal.
2' of coal
Primrose bed 22' thick
Landing
East
October 5th 1885
Position of F Bed or Primrose
Position of Mammoth
Tide
Greenwood Basin



allel line C E.



Plank Ridge Shaft.
al line C E

Henry A. Wasmuth
Instructor of Mining
University of Penna.

Fig. 27. Western Middle Coal Field. Mine Sheet No. 11. Sec. Geol. Survey of Penna. Section along prolonged Line C D. Scale 400 ft = 1 in.

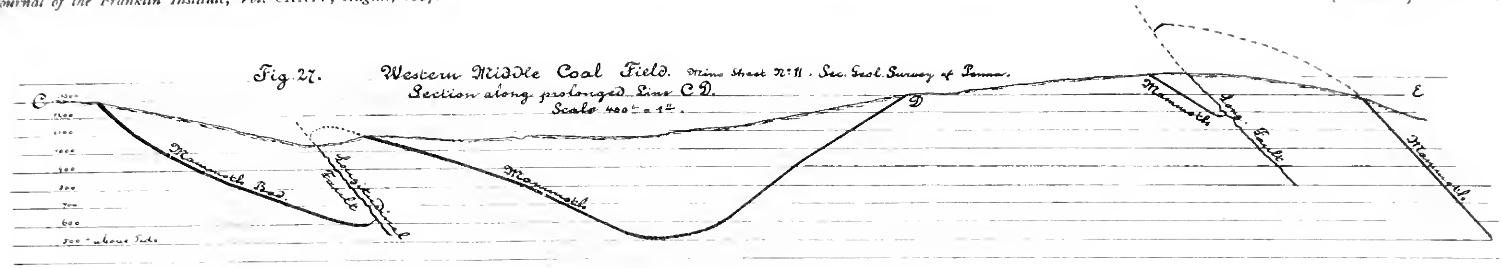


Fig. 28. Section A B through A and parallel C E.

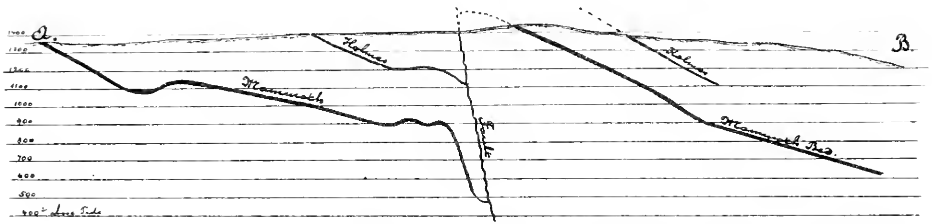


Fig. 29. Section F G through Kohinoor Shaft parallel line C E.

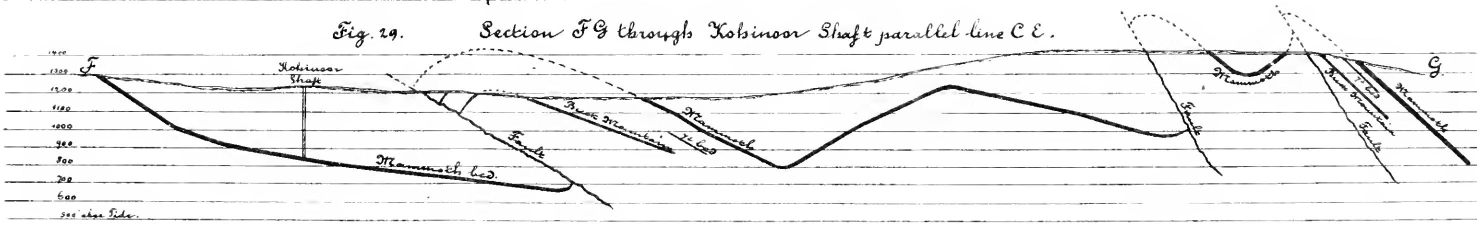
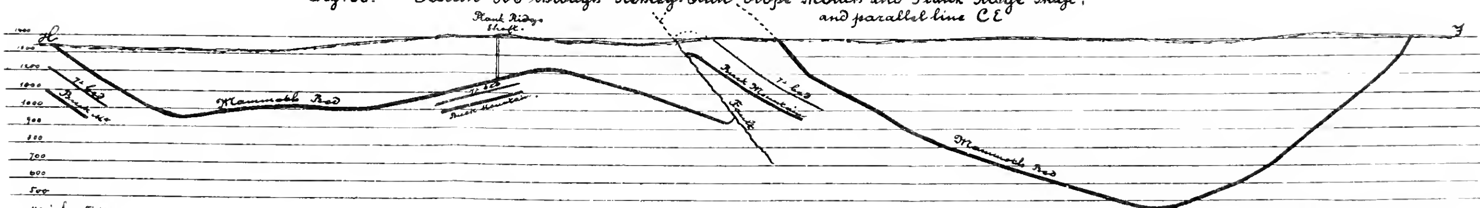


Fig. 30. Section H J through Kebley Run Slope Mouth and Plank Ridge Shaft, and parallel line C E



Henry A. Wasmuth
Instructor of Mining
University of Penna.

The bottom strata of the lost portion of the Holmes bed and parallel to its direction, have been struck directly on the hanging wall of the fault, and at the same time a gentle north-dipping flexure of the Holmes bed (anticlinal elbow), which terminates on the fault in the higher portion of the cross cut. The north-dipping flexure referred to, assumes a southerly dip of about 10° to 12° within a short distance, and sets down to the level of the cross cut or gangway, showing here its proper thickness. The bed forms several synclinals and anticlinals in its continuation, which have been intersected by the cross cut. [Statement of the mine boss.] The overlapping on the Holmes bed on the incline is about twenty to thirty feet. There is no room for even a suspicion of the existence of a short, inverted leg of the coal bed, *Fig. 3*, because if the fracture should represent the short leg, certainly there would be signs of parallelism of the coal bed and of its country rocks, too.

The dislocation of the Holmes bed necessarily must have been accompanied by dislocation of the lower Mammoth bed by the same fault, thus increasing the thickness of about twenty-three feet to about fifty feet as represented by line *c, d*, *Fig. 3*. The gentle rollings of the Holmes bed referred to, already have been or will be developed similarly in the workings of the Mammoth bed.

Numerous fractures of different dip and strike have been met with in the workings of the Mammoth bed, and also several gentle rolls. "A slickensides," about 2½ by 2 feet, has been exposed on the left rib of a gangway, on a gentle rising anticlinal, *B*, *Fig. 3*, the striation of which indicates a movement of the strata, obliquely to the lamination of the coal bed, thus strengthening the conjecture, that the local thickness of the Mammoth bed of about fifty feet near this striation has been produced by pushing and tossing of the gentle rolls of the strata *into numerous overlaps* of small extent by prodigious forces.

Middle Creek Colliery.—This colliery is one of the oldest operated in the Tremont district, and has been announced as having developed an "inversion," by its sinking, of two slopes on two different south-dipping flexures of the Mammoth bed within an horizontal distance of about 156 feet from knuckle to knuckle, and in an almost vertical plane. The ground plan of the mine, *Fig. 4*, has been constructed from data of memory, and from this section *Fig. 5*.

In the northern slope (*Fig. 5*), the coal bed terminated on rock at the depth of about 480 feet. The southerly slope, it has been proved, is on the same flexure of the Mammoth bed, worked from the level of the shaft.

The phenomenon has been explained to be an "inversion," as illustrated in *Fig. 6*, by all experts and officers in charge ever since. Assuming that the inversion exists here, then, according to the notions of the engineers, etc., as well as the illustrations of the State Geological Survey, the lower Buck Mountain bed must be inverted also, as illustrated in *Fig. 6*. The strata between the Mammoth and Buck Mountain beds is about 283 feet in Eckert Colliery near Tremont.*

In order to accommodate the doubling of the Mammoth and Buck Mountain beds, the strata of twice 283 feet = 566 feet at its greatest thickness, and considerable portions of the conglomerates of the bottom strata of the Buck Mountain bed also must have been almost squeezed out of existence.

If the rocks at the time of their disturbance have been as hard as they are to-day, is it supposable that such an overlapping could take place?

The persistency with which the present officers in charge insist that there is an inversion of the Mammoth bed, compared with the condition of the strata developed in mining, only permits the explanation that all the strata down to the Buck Mountain bed must have been pulled up and then shifted into a long, very narrow inversion.

But it has been proven beyond any doubt that no inversion has been developed in this colliery thus far, for: from the lowest or bottom gangway of the northern slope (*Fig. 5*) a cross cut toward the hanging wall has been driven for ventilating purposes, etc. It has not been reported yet that this cross cut has intersected the short, inverted legs of either the Mammoth or the Buck Mountain beds and their country rocks. An investigation of this cross cut was excluded, on account of water at the time of our visit.

From the bottom level of the shaft, which is about 700 feet below the levels of the knuckles of the slopes referred to, a cross cut has been driven from the Primrose to the Buck Mountain bed,

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* *Second Geological Survey of Pennsylvania.*

which crossed the Mammoth bed. About 308 feet north of the Mammoth bed, a distinct fault, filled by disintegrated coal and rock has been intersected by said cross cut, and within about seventy-seven feet farther north, several irregular fissures filled or lined with quartz. In this portion of the cross cut, the rock is very hard and without distinct stratification, but from here until the Buck Mountain bed has been intersected, the dip of the strata is about from 37° to 39° south. According to the theory on overlaps, advocated by the engineers of the anthracite region, as well as the illustrations of the State Geological Survey, the Buck Mountain bed should have been crossed at about 440 feet $\left(\frac{283}{sin\ 40^{\circ}}\right)$ *Fig. 6*, but the cross cut is about 625 feet long; that is, from the bottom of the upper member of the Mammoth to the bottom of the Buck Mountain bed. The greater length of the cross cut, about 185 feet, may be accounted for by a horizontal dislocation of about 156 feet, and the remainder by the local decreasing dip of the strata.

The non-existence of an inversion has been developed also by the east gangway on the Mammoth bed, illustrated in *Fig. 4*. The coal bed became contracted and, turning into a northerly course, it terminated completely. After several trial cuts, the top rock of the Mammoth bed (hard sandstone) and the lost portion of the bed was recovered by an oblique cross cut, about 160 feet long. The distance is about equal to the distance between the mouths of the two slopes on the Mammoth bed referred to, and also nearly equal to the distance of dislocation in the main cross cut. In the outside breast of the oblique cross cut, the coal terminated just as in the gangway, but I do not recollect at what height on the dip. A close investigation of the occurrence was prevented by the supporting timbering of the mouth of the oblique cross cut.

The east gangway on the Primrose has been stopped for some time, most probably in the same fault.

It has been demonstrated frequently, that such longitudinal faults seldom carry much water, but a great deal of mine gas on the strata of the hanging wall of the fault, which is explained by the shape of the flexure of the strata. This has been experienced to an extraordinary degree in the workings on the Primrose and Mammoth beds above shaft level.

The Otto Colliery.—Has operated during a long time above and below water level, two south-dipping flexures of the Primrose and Mammoth beds within a horizontal distance of about 1,300 feet.

The northerly flexure of the Mammoth bed, then called "White Ash Vein," has been worked by two adits above each other in a westerly direction for about one mile, and here cut off by rock. The very termination of the bed also has been reached at three lifts below water level. In the third level below water, the bed turned south and terminated about 350 yards west of the slope, *Fig. 7.* The ground plan of the mine has been constructed from memory. *Figs. 8* and *9* represent sections of it.

The southerly flexure of the Primrose and Mammoth beds have been operated in a westerly direction from Otto Colliery, and in an easterly direction from Swatara Colliery above and below water level to about equal horizons. The adits from Swatara Colliery on the Mammoth bed have been extended eastward for about 700 yards, then the bed turned towards the north and terminated on rock. The distance between the terminations of the corresponding water levels on the southerly flexure from Swatara Colliery, and on the northerly flexure from Otto Colliery is reported to have been very small, and their relative positions are explained to be on the contorted flexures of several anticlinal rolls, and as many synclinal basins.*

The east gangway on the southerly flexure of the Mammoth bed in the then lowest level of Otto Colliery had struck a fault with southeasterly strike. In this fault a so-called monkey gangway has been driven for more than 100 yards without recovering the lost portion of the Mammoth bed, *Fig. 7.* Then the fault was supposed to be an inversion, and they expected to recover the lost portion of the bed towards the north within a short distance; accordingly, a cross cut was commenced about 125 yards west of the intersection of the fault, *Fig. 7.* The narrow dimensions of this cross cut at the beginning confirm the stated supposition, yet to be investigated. The cross cut intersected regular south-dipping strata for about sixty feet from the gangway and then broken rocks, being about twelve feet thick in the upper part of the cross cut, and about four feet at its bottom. A distinct stripe of disintegrated rock, like a bench of yellowish clay, similar to a fault, is

* *First Geological Survey of Pennsylvania, i,* 434.

visible about the middle of the broken strata. The lower (synclinal) elbow of a contorted flexure of a thin coal bed is intersected just north of the broken strata, which terminates like the latter. From here, regular south-dipping strata only, with a number of coal beds, has been intersected by the cross cut and at about 1,350 feet from foot wall to foot wall, the then so-called "New Vein," or the lost portion of the Mammoth bed, identical with the northern flexure of the Mammoth bed, above referred to. The phenomenon has been defined to be an inversion by mining experts, and the officers in charge ever since (the fault was struck about twelve years ago), but, as we were informed by a very eminent mining expert at the time of our visit, it has not yet been clearly determined.

That the phenomenon is a longitudinal fault of considerable extent, is shown so distinctly as not to allow even a conception of an existing inversion; for, if it were an inversion, then the short, inverted legs of strata, not less than 600 to 700 feet thick, and including a number of naturally deposited coal beds, must have been compressed to a thickness of about four feet, or to the width of the broken strata referred to.

East Franklin Colliery.—From the bottom of the main slope, a narrow synclinal and an anticlinal have been developed within a distance of about 345 feet across the strata, *Fig. 10.* A number of oblong quartz druses, more exactly clefts, proofs of disturbance of the strata, have been intersected by the cross cut, *Figs. 10* and *11.* One of these clefts forms an outlet for considerable quantities of mine gas, thus proving a connection with over- or under-lying coal beds. The northerly flexure of the anticlinal is intersected by a distinct fracture, dipping about 70° north, and filled with disintegrated coal and rock, similar to a longitudinal fault. The coal of both flexures of the anticlinal above the level of the cross cut, is in a very fragmentary, friable state, running like quicksand, and perhaps, on account of this, the extension of dislocation by the fault referred to, has not yet been looked for. The sudden and great increase of the dip of the northern flexure of the coal bed, compared with the gentle dip of the strata of the foot wall of the fault, can be explained only as a result of the fault.

West Brookside Colliery.—Several transverse and one longitudinal fault have been developed in the workings of No. 4 slope.

The bottom of this slope is in the synclinal axis, which has been developed in the workings of No. 1 and No. 2 slopes.

In order to establish the east gangway on the north-dipping flexure, the top rock of the bed had to be stripped across the synclinal axis, by which "a transverse fault" has been crossed, *Fig. 12.* The fracture or fault dips about 70° south, is about two inches wide, and filled with disintegrated coal of a brownish color. The dislocated portion of the coal bed on the hanging wall of the fracture, is about ten inches lower than the portion of the bed on the foot wall of the fracture.

The east gangway on the south-dipping flexure has intersected a transverse fault, by which the portion of the bed on the hanging wall is about three feet lower than the portion of the bed on the foot wall of the fault, *Fig. 13.*

Both faults are of moderate extension, but exposed distinctly beyond any doubt as to their character.

A longitudinal fault (in the state of its origin) has been developed in breast No. 102 on the south-dipping flexure. There is no distinct fracture visible, but the bed is thicker than when sound and the coal in a fragmentary condition, notwithstanding that the dip of the bed is about 20°, *Fig. 14.*

A number of contractions of the bed have been crossed by the extensive workings, but they were not investigated during our visit.

Extensive longitudinal faults will be met with in the workings below the present deepest level of No. 3 slope, *Fig. 15.*

Williamstown Colliery.—*Fig. 15* represents a sketch of the ground plan of Williamstown and Brookside Collieries, and *Fig. 16* a section through the tunnel of this colliery. The main Lykens Valley bed has been in an exceedingly good condition down to the present deepest level. The strata of the northern part of the tunnel has been intersected by numerous fractures, some of them from six to eighteen inches wide, and filled with disintegrated rock, which will prove to be principally longitudinal faults of great extent; for the thickness of the strata between Lykens Valley and Mammoth beds is about 956 feet in Eckert Colliery near Tremont (*Second Geological Survey of Pennsylvania*); and if the thickness of the strata does not increase towards the west or Williamstown, then the tunnel should have intersected the principal

leader of the formation, the Buck Mountain bed, and also the Mammoth. Instead of crossing these beds, numerous splinters of coal beds only have been intersected, and the Mammoth bed crops out a little north of the tunnel mouth. There is no reason for the supposition of an increase of the thickness of the strata between the coal beds referred to, west of Eckert Colliery, because the northern flexures of the synclinal, from the Mammoth to the lowest Lykens Valley beds, have been explored at Klingers Gap, about two miles east of the northern tunnel mouth, but I do not recollect the distance between them; however, from the junction of Bear and Rausch Creeks, near which the Mammoth bed outcrops, the distance to the lowest Lykens Valley bed and red shale will hardly be more than 1,000 feet. The dip of the strata is here about 60° south, or nearly the same as the dip of the strata in the most northern part of the tunnel, and the Mammoth bed north- and south dipping flexures. Therefore, the great distance between the Lykens Valley and Mammoth beds across the tunnel must have been produced by dislocation of longitudinal faults, which is demonstrated by facts. The faults will be developed in the workings of Williamstown and Brookside Collieries below the present deepest levels, sooner or later, according to their general dip and strike; but if the dip of the Lykens Valley bed decreases in the future deeper levels of Williamstown and Brookside Collieries, certainly it will be the "forerunner" of the approach to the synclinal elbow of a longitudinal fault.

Sections of the Second Geological Survey of Pennsylvania.—Figs. 17 and *18* represent copies of Sections 2 and 3 of vol. i, of the Southern Coal Field, *Second Geological Survey of Pennsylvania.* According to its construction, the whole carboniferous formation has been pushed and tilted into one inversion and two sharp anticlinals, or into four synclinals and three anticlinals, inside of a horizontal distance of about 2,800 feet, without a single fracture.

The strata of this section of the coal field have been crossed by numerous adits, in which an unusually sudden replacement of the country rocks of the beds is demonstrated. For instance, the country rocks of the *F* or Primrose bed are transformed from sandstone into slate, and the reverse, within very short distances. A coal bed about eight feet thick, about 100 feet above the Primrose, has been squeezed out of existence in the so-called "Hell

Kitchen" overturned basin; it reappears in the next Greenwood basin and disappears again in Lansford basin.

Contractions of the Lykens Valley beds have been developed in Kalmia and Lincoln Collieries for very long distances, but no sudden and complete disappearance of the beds, nor sudden exchanges of the country rocks have been demonstrated; for, they are naturally deposited coal beds of small thickness.

The Holmes bed, eight feet of coal between *E* and *F* beds, has, as illustrated, doubled upon itself without fracture to a wedge-shaped large coal body in Rhume Run overturned anticlinal, which cannot take place, because the doubling upon itself would have to extend on the strike also for a certain distance and thus represent an instance as illustrated to occur in the great bed of Lorberry Creek, or an overlap in the bed itself with fracture (*First Geological Survey*, *i*, 237); consequently, if no overlap in the bed itself occurs without fracture, certainly overlapping of the whole strata cannot take place without fracture and more or less dislocation; for the latter conclusion is confirmed by the overlaps in Kohinoor, Middle Creek, Otto and Williamstown Collieries.

Based on these occurrences, I constructed from the Mine Sheets I and II, of vol. i, of the Southern Coal Field, *Second Geological Survey of Pennsylvania*, the sections, *Figs. 19*, *20*, *21* and *22*.

The Lehigh Coal and Navigation Company has lately sunk two shafts, and through them has been proved that the Greenwood basin and shaft anticlinal, as projected on the maps of the *Second Geological Survey*, are not in existence in that locality.

Figs. 23 and *24* are copies of maps of the *Industrial Statistics of Pennsylvania*, of 1885.

From *Fig. 23* it will be seen that the Primrose bed, or whatever its local name may be, has been inverted on its dip line for about 400 feet, without fracture.

Assuming the inversion exists, as illustrated, then the horizontal extension or strike of the bed must show a similar figure for a greater or less distance. Before inversion took place, all the strata have been in an undisturbed condition; consequently, the series of beds above and below the Primrose must have been tilted and folded into a very oblique inversion, and thus the lower Mammoth bed must have shown an unbroken inversion, too. A glance at the section will convince one that this perhaps is supposed to be

developed yet; but then, the cross cut passed regular south dips of the strata and then a pretty piece of coal (perhaps dirt), which is in a very pointed shape near the bottom of the cross cut. Where did this body of coal, indicated as a fragment of the splintered Mammoth bed, come from? It is obvious that it must be a fragment of the splintered Mammoth bed, and the splintering itself could be produced only by one or more fractures and dislocations.

Incidentally, the connection of the pointed coal body with the inverted leg of the Primrose bed forms a straight line, and there ought to be no doubt, that this line represents the fracture on which the strata on the foot wall of the fracture has slid down, thus producing a longitudinal fault or overlap, most probably through the whole carboniferous formation.

The dotted line, *D D*, indicates the bottom line of the Mammoth bed, constructed from the Mine Sheet III, *Second Geological Survey*, and no explanation is needed to prove that the Greenwood basin and the shaft anticlinal do not strike this location, as illustrated on the maps of the Survey.

Shaft No. 2, *Fig. 24*, passed from the surface down for about 150 feet on the anticlinal elbow of a longitudinal fault, or overlap, of a good-sized coal bed. (Strata of the hanging wall of the fracture.) From about 180 feet deep, south dips of the strata have been met, but the given data permit no conclusion, if, or where, a fracture has been struck. The dip of 55° has dropped to 26° within a very short distance, thus indicating a disturbance of the strata. The dotted lines, constructed from Mine Sheet II, *Second Geological Survey*, represent, approximately, the bottom lines of the Mammoth and Primrose beds. The latter fits well, provided the shaft is located about 250 feet southeast of the mouth of Tunnel No. 6. The dip of the strata through which the shaft passed, indicates that the existence of the Greenwood basin and the shaft anticlinal, as constructed on the maps of the *Second Geological Survey*, are entirely miscalculated, because all of the strata have been dislocated by one or several longitudinal faults of considerable extension.

Figs. 25 and *26* are copies of the Sections of the Map, "Part of the Mahanoy and Shenandoah basins, *Second Geological Survey of Pennsylvania*.

A probable "line of fault" is illustrated in section *C D, E F*, which is the *only one* supposed to exist in the anthracite region (at least according to the maps in possession of the University of Pennsylvania), and will be recognized, without any doubt, as a longitudinal fault or overlap. On the left or north side of this section, an overlap of about 500 feet on the dip, affecting the whole coal measures, is demonstrated. Now, if in the very section on the southern side "a line of faults" is necessary to explain the positions of two flexures of the Mammoth bed, why should there have been produced an overlap without fracture on the northern side?

Fig. 27 represents a section constructed from the corresponding Mine Sheet of the *Second Geological Survey*, on the theory hitherto advocated.

Fig. 25 represents a section about 11,600 feet east of section *C D*, in which the northerly overlap, already referred to, is constructed as a squeezing out of existence of considerable strata, which would be explainable only by the existence of "a dyke." The strata between Mammoth and Lykens Valley beds, in a thickness of about 740 feet, or 1,480 feet in all, have been squeezed to a thickness of about 200 feet within a distance of about 900 feet, which, if verified, would be a phenomenon heretofore not experienced in any part of the globe. I constructed section *Fig. 28*, from the corresponding Mine Sheet of the *Second Geological Survey;* but it is certain that the steep flexure, *a b*, of the Mammoth bed does not exist, because the gentle rollings of the bed, here two, have been developed to a greater number in Kohinoor Colliery, about 8,400 feet westwards, and it will be proved by the workings (if not developed already) that they exist in nearly the same number in Knickerbocker Colliery also.

Figs. 29 and *30* represent sections of the ground between the sections of *Figs. 25* and *26* of the *Second Geological Survey*, or of sections, *Figs. 27* and *28*, constructed by me, which need no further explanation than a comparison with my arguments on the matter.

It is a fact, that each mining engineer of the anthracite region has experience of some occurrences of overlaps, which proves the numerous occurrences of overlaps and the importance attached to them, and, furthermore, it will not be questioned that numerous

collieries have suffered by the abrupt termination of their coal beds, caused by longitudinal faults; and, therefore, the prevalent definition of longitudinal faults is most surprising. For instance, on p. 236, *First Geological Survey of Pennsylvania,* it is stated:

"The coal has rubbed upon itself and is in a more or less fragmentary, friable and polished state."

The polished state of the coal referred to, hypothetically admits as a fact, that the coal, and consequently the rocks also, at the time of their disturbance have been as hard as they are to-day; for, fragments of soft coal and rocks will never polish each other, and, therefore, a doubling upon itself of the roof, or the floor, or of both, as referred to, cannot take place without fracture and more or less dislocation. The doubling upon itself is also impossible; because the measures consist of a number of strata, of which *not one* can be shown to be inverted.

The existence of parallel displacements, or parallel, slips to an extraordinary degree in the coal beds of the Sharp Mountain (which, of course, must be accompanied by displacements on the strike also), has been reported by the *First Geological Survey* with the statement, that, if these parallel slips do not take place by the beds doubling upon themselves, they must have arisen from a fracture, *Fig. 2A.*

In the *Report of the First Geological Survey*, already referred to, is stated:

"It may be accepted as a general rule, not without its occasional exceptions, that these longitudinal faults consist of an up-throw of the vein to the S (South?) of the fault, so that the remainder of the vein, if we are mining downward, will be recovered at a point above and further back towards the outcrop, compared with the spot, where the rock wall crosses the mine."

Such a fault of considerable extent is illustrated by the section of the Kittatinny Valley (*First Geological Survey of Pennsylvania*), to occur in the blue slates near Wormleysburg, but none has been reported to occur in the anthracite region.

A comparison of the fault near Wormleysburg with the occurrences of longitudinal faults, developed in anthracite mining, a few of which are described before, and with the faults constructed on sections, *Figs. 19, 20, 21, 22, 27, 28, 29* and *30*, will show their great similarity, leaving no doubt at all, that overlaps have been

produced by dislocation on a fracture. But still, as many geological matters are enveloped in mystery yet, a short, inverted leg of an "*overturn*" in a coal bed may be found yet, although no such a thing, whether in the old extensive coal mines of Europe, or in anthracite mining in Pennsylvania has been developed thus far.

A comparison of the general rules applicable to faults, based on a long experience in mining in Europe will also show, that there is no room for the notion of the existence of a short, inverted leg of an overturn in a coal bed.

The origin of transverse faults in ore veins has been defined as in consequence of a sinking of the rocks on the hanging wall of the fracture or fault by Schmidt, Frankfurt, 1810; and this theory has been confirmed by numerous occurrences met with in "ore" and "coal" mining. (Zimmermann, Leipzig, 1828; *Engineering and Mining Journal*, of New York, vol. xxxiv, p. 56.) The rule to apply to such a fault is:

If a horizontal mine work strikes a fault dipping towards, cross the fracture and turn parallel to it towards the hanging wall; if the fault dips off, cross it and turn parallel towards the foot wall.

The origin of longitudinal faults has been defined by Heim, *Mechanism of the Strata*, Basel, 1878, and by G. Koehler, Professor of Mining at Clausthal, to be in consequence of disproportionate sinking, of different portions of the strata, thus causing first gentle synclinals and anticlinals, which became more or less uplifted, and finally the flexures were fractured and more or less displaced by prodigious forces, continuously acting.

The course of the fracture indicates to a certain degree the limits of disproportionate forces, and generally ranges nearly with the strike of the flexure and strata.

The definition stated above justifies the conclusion:

Whatever the course of the fault may be, the sliding down on the fracture of one portion of the strata, or even the disproportionate sliding of both portions, necessarily must be of two different characters.

If, for instance, a coal bed, *a b*, *Fig. 1*, is disconnected by a fault, *c d*, and either the portion *a* on the hanging wall of the fault, or the portion *b* on the foot wall of the fault, or even both slide down disproportionately, according to the height of sliding from their natural position, their appearance will be either as illustrated in *Fig. 1*, or as illustrated in *Fig. 2*.

In *Fig. 2*, the portion *a* on the hanging wall of the fault, is in a higher position, at the same time producing an overlapping of the measures, or *a longitudinal fault.*

In *Fig. 1*, the portion *a* on the hanging wall of the fault is in a lower position, and this is *a transverse fault.*

Dislocations, as illustrated in *Fig. 2*, are possible only when the strike of the fault ranges with the strike of the axis and strata, and the inclination of the fault, greater than the dip of the strata, is always of the course of the dip of the strata. They occur mostly in the neighborhood of anticlinals and synclinals, and have the same origin. Such faults are designated by the authorities referred to (Heim, Koehler) as the highest degree of folding, illustrated in *Fig, 31*, and it is evident that the portion on the foot wall of the fault has slid down. This explains that the cutting out of coal beds by longitudinal faults are generally preceded by contractions, and a bending towards the fault on the dip, as well as on the strike. The downward bending of the strata on the hanging wall of the fault (anticlinal elbow, *First Geological Survey*) may have been produced by the enormous weight of the overlying strata, and the prodigious oblique force of the sliding strata on the foot wall of the fault, while the up-bending (synclinal elbow, *First Geological Survey*) of the strata on the foot wall of the fault, may be explained by a certain resistance of the underlying strata, which necessarily must have produced an oblique movement of the strata in the direction of its strike with similar results. The rule to apply to a longitudinal fault is:

If a horizontal mine work strikes a fault dipping towards, cross towards the foot wall or bottom strata, if the fault dips off, cross towards the hanging wall.

Dislocations, as illustrated in *Fig. 1*, occur mostly with a strike of the fault, transverse to the strike of the axis and strata, they often cross anticlinals and synclinals, and therefore they are considered generally to be of later origin than the synclinals and anticlinals crossed by them. The portion of a vein or of a coal bed on the hanging wall of the fault has always slid downwards, even if the dip of the fault is in an opposite direction from the strata, as illustrated in *Fig. 32*.

The Maps of the *Second Geological Survey of Pennsylvania*, to a certain degree, are reconstructions of the mine maps; therefore,

the illustrations of overlaps without fractures, one single instance excepted, force one to the conclusion that either the mine maps do not represent the condition of the strata, or the survey has failed to realize its anticipations.

A SIMPLIFICATION OF THE NEW WEATHER SIGNAL CODE.

By Prof. Edwin J. Houston.

[*Abstract of Remarks made at the Stated Meeting of the* Institute *held Wednesday, May 18, 1887.*]

In the new code of weather-signal flags recently adopted by the United States Weather Bureau, approaching changes in the weather, as is well known, are indicated as follows, viz.:

The triangular black temperature flag indicates, by its position above or below the clear weather (a white square flag) or the precipitation flag (a blue square flag), a higher or lower temperature, respectively; thus, *Fig. 1*, indicates clear weather, higher temperature; while *Fig. 2* clear weather, lower temperature; *Fig. 3* rain or snow, with higher temperature; *Fig. 4* rain or snow, with lower temperature. The cold-wave flag, *Fig. 5*, a square black

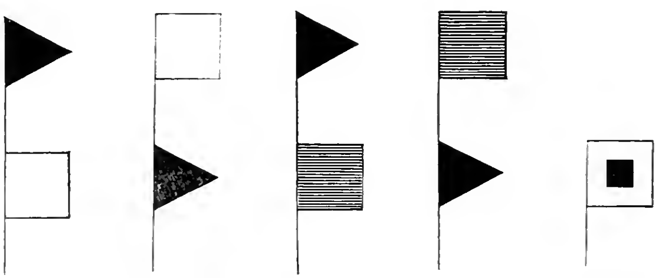


Fig 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5.

centre on a white ground, is displayed whenever a decided fall in temperature is indicated. It is usually displayed at least twenty-four hours before the fall in temperature is expected to come.

The cold-wave flag, of course, is never displayed along with the temperature flag. The other three flags, however, may be dis-

played simultaneously. In this case, when displayed from poles, the indications of the upper flag are to be taken first. When displayed horizontally (see *Fig. 6*), the left-hand flag is to be taken first, indicating rain or snow, followed by fair but colder weather.

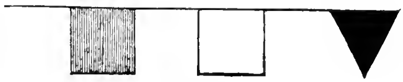


Fig. 6.

The author proposes a simple modification of the above code, which, while it will do away with the cold-wave flag entirely, will, nevertheless, permit the indications both of a cold and a hot-wave when it is so desired.

This modification is, briefly, as follows: while the triangular temperature flag is employed as before to indicate warmer or colder weather, respectively, according to its position above or below the clear weather, or the precipitation flag, the extent of the change towards heat or cold is indicated by the distance it is placed above or below these flags.

According to the code as now employed, the separate flags are displayed separated from one another a distance equal to once their diameter. In the code as modified, it is proposed to do away with the cold-wave flag and replace it by the triangular temperature flag displayed below the clear weather or the precipitation flag, a distance equal to at least twice the diameter of any of the separate flags. Thus, *Fig. 7* indicates clear weather; cold wave.

Fig. 7.

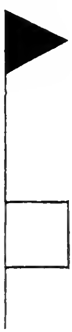


Fig. 8.

Fig. 9.

Fig. 10.

In the same way the triangular temperature flags, when displayed

above the other flags, at a distance equal to at least twice the diameter of a single flag, indicates an approaching hot wave. Thus, *Fig. 8* indicates clear weather; hot wave.

Since the poles or horizontal supports for the signal flags, as now employed, provide sufficient space for the simultaneous display of three flags, no difficulty will be experienced in obtaining the distance of twice the diameter of a single flag as required by the proposed modification.

No difficulty need be anticipated in distinguishing between the lower temperature indicated, and that of a cold wave as indicated in the two flags (*Figs. 9, 10*), since whatever the distance of the observer, the apparent size of the flags will always readily permit a ready decision as to whether they are at a greater distance apart than once their diameter.

It is evident that, when so desired, the severity of the hot or the cold wave can be indicated by displaying the temperature flag a distance above or below the other flags greater than twice their diameter. Or, should such refinement become desirable, distances equal or proportional to the diameter of a single flag might be marked off on the support, to indicate differences of 5°, 10°, or 20°.

The author believes that the excellent code now in use would be greatly improved and simplified by the modifications he proposes.

CENTRAL HIGH SCHOOL,
Philadelphia, June 24, 1887.

NOTE ON LONG COLUMNS.

By WM. CAIN, Prof. Math. and Eng., S. C. Mil. Academy.

(*Concluded from page 61.*)

ANOTHER FORMULA, INVOLVING THE BREAKING STRESS OF THE MATERIAL.

To point out the difficulties of the subject, it may be well to deduce a formula that shall include the breaking stress of the material, and that will lead, on certain suppositions, to the well-known Rankine form, whose insufficiency can thus more clearly be realized.

As in practice there is nearly always some eccentricity in the position of the load, whose influence is very marked, particularly in the case of short columns, where theoretically there can be no bending except for this eccentricity, we shall include it from the start in the formula.

Thus, in the figure, let the load, P, act at a distance, k, to the left of the centre line of the column, $A B$, which, after strain, bends to the curve, $A' D B$; then calling, as before, $f = C D$ the deflection at the centre from the axis, the moment of P with respect to D will now be $P(f+k)$. We shall further designate by Q the area of the cross section at D, by z the distance from the centre of gravity of the section to the most compressed fibre, and by E, I and r the modulus, moment of inertia and radius of gyration of the section about a horizontal axis through D, perpendicular to the plane of the paper.

Now, if we conceive applied at D, the centre of gravity of the cross section, two opposed forces, both equal to and parallel to P, then the downward force at D causes a uniformly distributed stress over the section Q of intensity,

$$p_1 = \frac{P}{Q};$$

and the upward force at D with the load P, k inches to the left of A, form a couple, whose moment is $P(f+k)$, which causes a

uniformly varying stress in the section, whose greatest intensity, at the most compressed edge, is

$$p_2 = \frac{P(f+k)z}{I}$$

The greatest compression per square unit at the most compressed edge of the section at D is, therefore,

$$p_0 = p_1 + p_2 = \frac{P}{Q} + \frac{P(f+k)z}{I}$$

in which expression, if P represents the breaking load, p_0 represents the ultimate compressive unit stress, supposing, approximately, the law "*ut tensio sic vis,*" holds up to the failing point.

Putting $I = Q r^2$, we can write the above formula,

$$p_0 = \frac{P}{Q}\left(1 + \frac{(f+k)z}{r^2}\right) \qquad (10)$$

Now we shall introduce *our first approximation* by supposing the curve $A' D B$ to be *an arc of a circle* of radius ρ, so that we have, calling l the length of the column,

$$f(2\rho - f) = l^2 \;\therefore\; \rho = \frac{l^2}{2f} \textit{ nearly.}$$

The last relation is sufficiently true for flat arcs, and is exactly true for the radius of curvature at D, if we regard the arc $A' D B$ as parabolic.

Next let us consider two cross sections after strain at D, ds apart along the axis, then if a = change in length of a fibre parallel to the axis, one unit long and at the distance z from the axis of the column, due to the bending moment alone, we have the well-known relation,

$$\rho : ds :: z : a\,ds \qquad \therefore\; \rho = \frac{z}{a}.$$

Equating this value of ρ with the previous one, and noting that $a = \frac{p_2}{E}$, we have

$$\frac{z}{a} = \frac{l^2}{2f}$$

$$\therefore\; f = \frac{l^2 a}{2z} = \frac{l^2 p_2}{2Ez}; \qquad (11)$$

which, substituted in the value for p above, gives

$$\frac{P}{Q} = \frac{p_0}{\left(1 + \frac{k\,z}{r^2}\right) + \frac{p_2}{2\,E}\left(\frac{l}{r}\right)^2} \qquad (12)$$

This formula, within the limits of elasticity, is perfectly correct, provided the elastic curve is taken approximately as a circle or parabola; but as we know neither k nor p_2, we may introduce errors just at this point by regarding them both as constant for all length ratios; for although k may be nearly constant for the same shapes and end connections and care in fitting, yet $p_2 = \frac{P(f + k)z}{I}$ varies with f and probably is not constant for all length ratios. Placing $\left(1 + \frac{k\,z}{r^2}\right) = a$, and $\frac{p_2}{2\,E} = b$, (12), can be written,

$$\frac{P}{Q} = \frac{p_0}{a + b\left(\frac{l}{r}\right)^2} \qquad (13)$$

In the well known Gordon-Rankine formula the load is not supposed eccentrically placed, so that $k = o$ and $a = 1$; also p_2 is regarded as constant (which is doubtless erroneous) and b is treated as a constant to be determined by experiment.

The writer first pointed out in *Van Nostrand's Magazine*, for November, 1877 (in criticising the Hatzel formula), that b was not necessarily a constant, though it is remarkable how near the results agree in a general way with experiments at the breaking limit for values of $\frac{l}{r}$ varying from 30 to 600, upon the supposition that b is a constant. It is much more rational though not to assume the eccentricity of the load zero, but determine, from the average of the great number of experiments that have been made, an average value for a, and likewise ascertain whether b can be treated practically as a constant or otherwise (see *Weisbach's Mechanics*, vol. i, p. 545, for a similar determination). Now, a difficulty presents itself at once in pursuing this plan, namely, the determination of p_0 for wrought iron, for its value has been given all the way from 36,000 to 60,000 pounds.

I believe the value 36,000, given in Gordon's formula, to be

erroneous, since Fairbairn and others have shown by experiments on transverse breaking of wrought-iron beams, properly designed at the compression flange, so as to give way there, if at all, by direct crushing, and not by bending, that the tensile unit stress (exerted at the lower flange) was practically equal to the compressive unit stress exerted in the top flange. If the tensile unit strength of the iron was, say, 55,000, then we should place $p_0 =$ 55,000 in the formula. In fact, I find that for $p_0 = 56{,}000$, $a = 1{\cdot}4$ and $b = \frac{1}{20000}$ for wrought-iron columns, with fixed ends, that the formula strikes an excellent average of the results given on Mr. Wilson's diagram (see *Trans. Am. Soc. Civ. Engs.*, vol. xv,) for length ratios varying from 0 up to 240. For higher length ratios, b should gradually increase from $\frac{1}{20000}$ to $\frac{1}{15000}$ (see Mr. Johnson's paper, mentioned further on). The formula is very flexible; for not knowing p_0, a or b, we can determine them to suit the results between any limits, though of course *the formula becomes largely empirical by so doing*. Thus the values $p_0 = 50{,}000$, $a = 1{\cdot}2$ and $b = \frac{1}{20000}$ for *wrought-iron* columns with *fixed ends* will agree fairly with experiments from $\frac{l}{r} = 0$ to $\frac{l}{r} = 500$, though the agreement for the smaller length ratios is not so good as on the first assumption. Similarly the values $p_0 = 50{,}000$, $a = 1{\cdot}1$ and $b = \frac{1}{10000}$ will approximate to the average of the experiments on *wrought-iron columns, hinged ends*,* though not nearly so well for $\frac{l}{r} < 240$ as for higher length ratios where a good average is struck.

For *cast-iron* columns with *fixed ends*, the values assumed were $p_0 = 100{,}000$, $a = 1{\cdot}2$, and $b = \frac{1}{5000}$, which gave excellent results for all length ratios.

Some of the principal results are recorded in the following table:

Wrought-Iron Columns, Fixed Ends.

$\frac{l}{r}$	0	50	100	150	200	300	400	500
$\frac{P}{Q}$	41667	37700	29400	21500	15600	8770	5430	3650

* The eccentricity of the load in the case of "hinged ends" is only realized when friction between the pin and column is exerted, and its *extreme limit* is easily found, as in the case of trunnions, by known laws of mechanics.

Wrought-Iron Columns, Hinged Ends.

$\frac{P}{Q}$	41667	37000	23800	15000	9800	4950	2900	1900

Cast-Iron Columns, Fixed Ends.

$\frac{P}{Q}$	83333	58800	31300	17600	10900	5200	3000	2000

For $a = 1{\cdot}2$, as found for fixed ends, we have $k = 0{\cdot}2\frac{r^2}{z}$, from which we can determine the eccentricity of the load for an assumed cross section. Thus, on substituting the well-known values of r, the radius of gyration, and z, the distance from the axis to the most compressed edge of the column, we find that for *rectangular sections*, $k = \frac{1}{30}$ depth; for *solid cylindrical* columns, $k = \frac{1}{40}$ diameter; for *thin, hollow* cylinders, $k = \frac{1}{20}$ diameter; for thin, hollow, square columns, $k = \frac{1}{15}$ depth; and for *common columns*, formed of two channels connected by latticing, which give way in the direction of the latticing, $k = \frac{1}{10}$ depth of latticing. Although the eccentricity is thus found to be small, it can be objected that the formula gives different eccentricities to different shapes whose ratio is seemingly arbitrary, for which there can be no good reason. A separate formula, then, for each shape would alone answer the objection. The same objection holds, however, with respect to any *one* formula that may be devised for all shapes, and is not very serious when a rough average of the whole is alone desired.

For the hinged ends, where $a = 1{\cdot}1$, k has one-half the values given above. In cases where the direction of P is not parallel to the axis, or even crosses it, k would still represent the eccentricity at the dangerous section.

We note that, for wrought iron, as $\left(\frac{l}{r}\right)$ approaches the limit zero, that the *average* intensity of stress on the cross section $\frac{P}{Q}$ approaches the value 41,667 pounds, which is only a fractional part of the assumed greatest intensity, 50,000 pounds, as it should be. Similarly for cast iron.

The form proposed would, therefore, seem to be far preferable to the ordinary Gordon form in this respect, though both labor under the objection of being semi-empirical in character.

It may then be inquired whether a simple, purely empirical formula cannot be devised from the experiments that will give better results than the Gordon and allied forms?

In the discussion on Mr. Wilson's paper, before mentioned, there were certain empirical formulæ proposed, as embodying the average results of experiments on wrought iron, by E. Thacher, M.Am.Soc.C.E., which, within the limits, $\frac{l}{r} = 20$ to 200, are found to give as good or better results than more complicated formalæ.

This whole subject, however, has been so fully presented by Thomas H. Johnson, M.Am.Soc.C.E., in a paper "On the Strength of Columns," read at the Annual Convention of the American Society of Civil Engineers, June 26, 1885, that one has but to glance at the admirable diagrams given in that paper, to see that the kind of formulæ proposed by Mr. Johnson represent about as nearly as is possible for any set of formulæ to represent, the average of the great number of experiments that have been made on the breaking weights of columns.

In the numerous diagrams given, the abscissas represent the ratio of the length to the least radius of gyration $\left(\frac{l}{r}\right)$ and the ordinates, the corresponding average breaking stress $\frac{P}{Q}$ on the cross section.

Mr. Johnson found for all materials that "(1), that part of the line corresponding to the higher length ratios, is a curve, the equation of which is Euler's formula; (2), that part of the line corresponding to the lower length ratios, is a straight line tangent to the aforesaid curve, and intersecting the vertical axis at a point which is constant for each material in all the varieties of end bearings; (3), the ordinate at the point of tangency is one-third of that part of the vertical axis intercepted by the tangent."

The second law above is in agreement with the proposed formulæ of Mr. Thacher for wrought iron, as far as expressing the average breaking stress as a linear function of the length ratios is concerned. Mr. Johnson has drawn curves for maximum and minimum, as well as average values, and expressed likewise by the formulæ given below, for various materials, the average stress $\frac{P}{Q}$

on the cross section at the breaking limit for various ratios of l to r.

For the sake of brevity, let us call,

$$p = \frac{P}{Q} = \frac{\textit{breaking load in pounds.}}{\textit{cross section in inches.}}$$

$$x = \frac{l}{r} = \frac{\textit{length of column in inches.}}{\textit{radius of gyration of cross section in inches.}}$$

$x' = \left(\frac{l}{r}\right) =$ *abscissa of point of tangency below which the equation of the tangent is used and for greater values of x the curve of Euler's equation.*

We give in the table below the special forms of the equations for various materials and end bearings, observing that the experiments on oak and oolitic limestone are comparatively meagre; but it is understood in formulæ of this kind that the constants are to be amended from time to time as experience dictates. For wrought iron, the formulæ embrace the average results of a very large number of experiments for every style of cross section and extending up to 500 radii of gyration.

On looking over these formulæ, which largely embody the results of experiments, the question naturally occurs, Why does Euler's formula give the crippling load for very long columns, but not for the shorter ones? The answer is, in a general way, that in the first case the limit of elasticity has not been exceeded when bending just occurs; in the last case it has been. Thus, for columns hinged or round at both ends, we have for $f = 0$ from (7), since the quantity in brackets is equal to zero, and making $i = 1$,

$$\frac{2\,a}{r} = \frac{\pi\sqrt{\frac{e}{P}}}{\sqrt{1 - \frac{P}{e}}} = \pi\sqrt{\frac{e}{P}} \text{ nearly,}$$

in which $\frac{P}{e} = \frac{P}{E\,Q} =$ shortening of column per unit of length. For *wrought iron*, take $E = 27{,}000{,}000$, and *at the elastic limit* the unit stress, $\frac{P}{Q} = 27{,}000$, which, in the above formula, gives $\frac{2\,a}{r} = 100$ nearly. Now, if we suppose all the terms of the

MATERIALS.	End Bearings.	Equation of Tangent.	$x' = \frac{l}{r}$	Equation of Curve.
Wrought Iron.	Flat,	$p = 42000 - 128\,x$	218·1	$p = \frac{666,090,000}{x^2}$
$E = 27,000,000$	Hinged,	$p = 42000 - 157\,x$	178·1	$p = \frac{444,150,000}{x^2}$
	Round,	$p = 42000 - 203\,x$	138·0	$p = \frac{266,490,000}{x^2}$
Mild Steel. (Carbon = 0·12.)	Flat,	$p = 52500 - 179\,x$	195·1	$p = \frac{666,090,000}{x^2}$
$E = 27,000,000$	Hinged,	$p = 52500 - 220\,x$	159·3	$p = \frac{444,150,000}{x^2}$
	Round,	$p = 52500 - 284\,x$	123·3	$p = \frac{266,490,000}{x^2}$
Hard Steel. (Carbon = 0·36.)	Flat,	$p = 80000 - 337\,x$	158·0	$p = \frac{666,090,000}{x^2}$
$E = 27,000,000$	Hinged,	$p = 80000 - 414\,x$	129·0	$p = \frac{444,150,000}{x^2}$
	Round,	$p = 80000 - 534\,x$	99·9	$p = \frac{266,490,000}{x^2}$
Cast Iron.	Flat,	$p = 80000 - 438\,x$	121·6	$p = \frac{394,720,000}{x^2}$
$E = 16,000,000$	Hinged,	$p = 80000 - 537\,x$	99·3	$p = \frac{263,200,000}{x^2}$
	Round.	$p = 80000 - 693\,x$	77·0	$p = \frac{157,920,000}{x^2}$
Oak. $E = 1,200,000$	Flat.	$p = 5400 - 28\,x$	128·1	$p = \frac{29,604,000}{x^2}$
Oolitic Limestone. $E = 4,350,000$	Flat,	$p = 9000 - 32\,x$	189·1	$p = \frac{107,314,500}{x^2}$

formula to remain constant but the length of column $2a$, we see that an increase of length in formula (7) will give some deflection for the same P, so that bending will *just* begin for a *less* uniform compressive stress than the 27,000 assumed; therefore, when the column begins to fail by bending for $\frac{2a}{r} > 100$, the limit of elasticity has not been passed, at the instant when bending begins, so that Euler's formula should exactly apply, provided all the theoretical hypotheses (of a straight column of homogeneous material, with a load acting along the axis and $i = 1$) are exactly realized. The reverse holds for shorter columns; thus, for $\frac{2a}{r} = 77$, we find that $\frac{P}{Q} = 45,000$ pounds, or the elastic limit has been long exceeded before the column, theoretically, begins to bend at all. If 45,000 pounds was the crushing unit stress, we see for this ratio, $\frac{2a}{r} = 77$, and for all lower length ratios that the column should, theoretically, give way by crushing entirely, of uniform intensity over the whole cross section, as no bending is possible if the original hypotheses are realized.

In a similar manner, we find the least values of $\frac{2a}{r}$ for which the elastic limit has not been passed when bending is just about to begin, and Euler's formula becomes applicable—for *mild steel*, 87; *hard steel*, 67; *cast iron*, 70, using the moduli given in the table above, and regarding the elastic limits in order: 35,000, 60,000 and 32,000 pounds.

For columns with *fixed ends*, these ratios are all doubled, as we easily deduce from the formulæ corresponding to fixed ends, given above.

These results show, too, in a general way, that the inapplicability of Euler's formulæ to usual column lengths, as shown in the table above, is not entirely, or even principally, due to "the innate perversity of inanimate things," but rather that the hypotheses of perfect elasticity, etc., upon which the theory is founded, no longer, even approximately, holds.

The empirical formulæ proposed answer all the needs of practice, though it is possible that formula (13) may answer better for

any one shape; but where a general average only is desired, the linear equations above cover nearly all practical cases, and they are, of course, preferable to Euler's formulæ, which give values entirely too large for small length ratios; also, to the Gordon-Rankine form, not only on account of their simplicity, but because for very small length ratios they are more accurate, and for medium length ratios equally as accurate, as the partly empirical formulæ they are intended to replace; on which accounts the above empirical formulæ are cordially recommended for practice.

APPENDIX.

In *Executive Document* 12, 47th Congress, 1st Session, is given the Watertown experiments on posts of white and yellow pine, varying in size from about 5·2 inches square to a cross section of 8·5 x 16·5 inches and up to 62 diameters in length. (Also, see Lanza's *Applied Mechanics*, pp. 511–519.)

On plotting the ascertained unit stresses corresponding to given ratios of length to least side of the rectangular cross section $\left(\frac{l}{d}\right)$, it is found that the *average* unit stresses $\frac{P}{A}$ are approximately given by the following formulæ up to $l = 60\,d$ nearly:

$$\text{for } \textit{white pine}, \quad \frac{P}{A} = 3000 - \frac{100}{3}\left(\frac{l}{d}\right), \text{ and}$$

$$\text{for } \textit{yellow pine}, \quad \frac{P}{A} = 5500 - \frac{200}{3}\left(\frac{l}{d}\right),$$

in pounds per square inch of cross section.

The *minimum* unit stresses are about 400 pounds less for corresponding values of $\frac{l}{d}$ with but few exceptions. There were sixty-six experiments on white pine single sticks (which alone were considered), and seventy-two tests of the yellow pine posts.

It is hardly probable that in practice the end fittings are so perfect as in the Watertown tests, so that, although the above formulæ probably give us the average of the most complete tests that have yet been made on pine posts, yet a certain discretion must be used in applying them.

According to Mr. Johnson's rule, it will be found that for *white pine*, Euler's formula would have to be used for $\left(\frac{l}{d}\right)$ greater than 60, for which length ratios,

$$\frac{P}{A} = \frac{3{,}600{,}000}{\left(\frac{l}{d}\right)^2};$$

and for yellow pine the limit is $\frac{l}{d} = 55$, beyond which,

$$\frac{P}{A} = \frac{5{,}544{,}825}{\left(\frac{l}{d}\right)^2}.$$

But it must be carefully borne in mind that no experiments can be appealed to to substantiate these last values, which are much higher than sometimes given.

It should be noted that for "round" ends, Mr. Johnson used Euler's exact formula in the table above, but the experiments on iron showed that for flat ends, the coefficient $4\,\pi^2 = 39{\cdot}48$ of Euler should be changed to $24{\cdot}67$, which is probably on account of the unknown eccentricity of the load.

THE FUTURE WATER SUPPLY OF PHILADELPHIA.

In Chief Ogden's report of the operations of the Water Department of the City of Philadelphia, for the year 1886, which was submitted to Councils, on Thursday, July 14th, is given the final report of Rudolph Hering, engineer in charge of the "Surveys for the Future Water Supply." These investigations were begun in May, 1883, and continued during the years 1884, 1885 and until July, 1886. There was expended for the surveys $81,547.96.

In the course of his recapitulation of "conclusions that have been arrived at from the examinations," Mr. Hering says:

"In making these investigations, it has been taken for granted from the outset that the water from any point in the Schuylkill River, and from any point in the Delaware River, below Trenton, will not be of a sufficient good quality to furnish a future supply for the city, although the fact has been admitted that, at present,

the Delaware water at Lardner's Point, within the city limits, is not only fairly good, but is likely to remain so for some time.

"In looking about for an improved supply, every practicable scheme was considered. No success could be expected from a supply by artesian or driven wells in this locality, nor would filtering or purifying the water of the Schuylkill or lower Delaware give permanent satisfaction. The only schemes worth investigating were those which bring to the city the water of running streams in the Schuylkill, Delaware or Lehigh water-sheds.

"It required but little thought to see that the water from the streams north of the Blue Mountains would be the best available in quality not only now, but for an indefinite future, and that this region would therefore have to be the ultimate source of water supply for Philadelphia, and probably also for other cities lying between the mountains and the seaboard.

"To obtain an intelligent opinion on the cost of such a supply, surveys and examinations were made, which showed that inasmuch as water of good quality can be secured at a less expense from nearer localities, it is not advisable at once to go to the Blue Mountains.

"In adopting a scheme for an earlier future, this ultimate source, however, should be considered, so that the aqueducts now constructed could be available for the final source of supply. The quantity of water which it was thought best to calculate for at present was at least 200,000,000 gallons per day, or more than double the present consumption. The elevation at which the water should be delivered was fixed at about 170 feet above datum (the height of the present basin at Wentz's farm and the proposed basin at Cambria), because it gives the most favorable distribution for the city.

"The streams offering a good water supply nearer than the Blue Mountains are the Perkiomen Creek, a tributary of the Schuylkill River, the Tohickon and Neshaminy Creeks, tributaries of the Delaware River, and the Delaware River itself, above Trenton. In point of quality the water of the latter has been found to be the best; that of the Upper Perkiomen and Tohickon Creeks comes next in quality; and that of the Neshaminy and Lower Perkiomen Creeks is least good.

"An estimate of the cost of obtaining Delaware water alone

indicates that above Lardner's Point the most economical scheme is to bring it from Point Pleasant, because the river has quite a descent near this place, which materially reduces the height of pumping as compared with points lower down the river, such as Lumberville, New Hope and Yardleyville. Another advantage gained by this sudden descent is the water-power, which can be developed to furnish a daily supply of 120,000,000 gallons during the dry season.

"The cost of aqueduct, pumping plant and capitalized cost of pumping, amount to $19,622,543, if 210,000,000 gallons of water daily are pumped by steam, and to $15,475,262 if only 120,000,000 gallons are pumped by water and the remainder by steam.

"Purely gravity supplies, without pumping, can be obtained from either the Perkiomen Creek or from the Tohickon and Neshaminy Creeks combined. The latter project cannot be made to furnish a daily supply of over 156,000,000 gallons in years of minimum rainfall. While the water furnished by the Tohickon and Upper Perkiomen Creeks is good, that which is taken from the Neshaminy and Lower Perkiomen will be of much inferior quality. Neither of these purely gravity schemes would therefore be quite satisfactory.

"The cost of procuring a supply from the Perkiomen Creek is $13,674,493, and from the Tohickon and Neshaminy Creeks together, $13,846,662.

"Finally, a combined gravity and pumping scheme is possible by procuring water from the Tohickon Creek and from the Delaware River at Point Pleasant. The former can furnish on the average between 90,000,000 and 100,000,000 gallons per day by gravity; in minimum years only 80,000,000 gallons can be depended upon. The Delaware River, as we have seen, can furnish 120,000,000 gallons by water-power. Both the Tohickon and Delaware waters have been found to be not only of good quality, but much better than the waters of the Neshaminy, and particularly of the Lower Perkiomen Creeks. The cost of this scheme is $12,695,941, if the water-power is utilized, and $17,717,025, if steam-power is used.

"It is, therefore, clear that the best and most economical project to supply the city of Philadelphia with water is to bring to it the Tohickon water by gravity and to pump from the Delaware River at Point Pleasant by water-power.

"In order to perceive the relative values of the different schemes with still more distinctness, I have made three estimates; one, for completely filling the aqueduct; one, for furnishing 150,-000,000 gallons, and one for only 90,000,000 gallons per day.

"To supply the latter quantity of water from the Perkiomen Creek requires an expenditure of $10,495,000. In bringing 90,000,000 gallons daily from the Delaware water-shed, it is found that the Neshaminy Creek alone could furnish the amount, except during years of minimum rainfall, at a total expense of $7,875,000. The Tohickon Creek also could furnish a quantity up to 90,000,000 gallons, except during very dry years, at a cost of $10,008,000. If the Delaware water at Point Pleasant is used, the cost for 90,000,-000 gallons is $12,775,000, if pumped by steam, and $9,673,000, if pumped by water-power. At Lardner's Point, the cost would be $7,064,000.

"Therefore, to supply the city with 90,000,000 gallons daily of good water, which is the present consumption, the cheapest project is to pump the Delaware water at Lardner's Point; the next is the Neshaminy scheme, and the third is pumping Delaware water at Point Pleasant.

"To increase the supply to 150,000,000 gallons requires a total expenditure of about $12,139,000, if the Perkiomen water only is used, and a total expenditure of about $17,635,000, if no water is taken below Green Lane, and the deficiency supplied from the eastern affluents of the Lehigh River above the Lehigh Gap.

"On the Delaware areas the water stored from the Neshaminy and Tohickon Creeks together could furnish an amount up to 156,-000,000 gallons at a cost of $13,846,662. If, instead of using the Neshaminy water, Delaware water is pumped at Point Pleasant, the cost would be $14,275,000, if steam, and $11,215,000 if water-power is employed. To supply Delaware water only would cost, if pumped by steam at Point Pleasant, $16,355,000, and at Lardner's Point, $10,415,000.

"For supplying 150,000,000 gallons daily, therefore, from beyond Lardner's Point, the project contemplating the use both of the Tohickon and Delaware water at Point Pleasant, pumping the latter by water-power, is the least expensive one.

"Finally, to increase the supply to 210,000,000 gallons, the Point Pleasant scheme, as already stated, is again the most economical one, besides furnishing decidedly the best quality of water.

"It, therefore, appears with sufficient clearness, I think, that whenever good water can no longer be obtained from Lardner's Point by the pumps which it may be considered advisable to place at this point, the city should build an aqueduct to Point Pleasant, pump Delaware water by water-power, and supplement the quantity as it may become necessary, by storing the water from the Tohickon Creek, first in the lower, and then in the upper reservoir.

"After the aqueduct is taxed to its full capacity, at which time it will probably be necessary to go to the Blue Mountains for an increased supply, another aqueduct will have to be built. It is premature, I think, to say definitely at present whether this second aqueduct extending to the Blue Mountains should go by way of the Delaware or the Lehigh River. If the South Mountain region should preserve its present character, there can be no doubt that it should extend by way of the Perkiomen Valley, and, after receiving the South Mountain water at Green Lane, follow up the Lehigh River. The cost of this scheme, which now is relatively greater than that of others, would then probably be less. The Point Pleasant aqueduct could later also be carried to the mountains whenever the quality of the water, owing to the pollution from the Lehigh River, becomes objectionable; and its extension would then most economically be to the Delaware Water Gap.

"It is better to build two separate aqueducts in this way than only one with double the capacity, because in the latter case the risk from accident becomes greater. New York, Boston, Washington and Paris, have each two. London has even more.

"When the above-mentioned aqueducts are built, the city of Philadelphia will be supplied with the best water obtainable in Eastern Pennsylvania."

THE REACTION OF A LIQUID JET;

Being a Review of § 522 and § 523 of Weisbach's „Ingenieur- und Maschinen-Mechanik, Erster Theil; Fünfte verbesserte und vervollständigte Ausgabe, Braunschweig, 1875;"
With some additional matter.

BY PROF. J. BURKITT WEBB, Stevens Institute, Hoboken, N. J.

This article is intended principally as an explanation of an author belonging to those few who have earned the right to be regarded as authorities, and of a method which should commend itself to all those who see in the law of conservation of energy, one of the surest bases of analysis. It is to be regretted that criticisms of such authors are often made, which a thorough examination of the subjects criticised might show to be unwarranted, and in this way, the respect due to their authority may be unjustly weakened; I hope that the tendency of this article may be in the contrary direction, and that the slight corrections, which many students, no doubt, would make for themselves, may be found reasonable by all, and may tend rather to contribute to than to detract from the value of a work, the almost-completed recent review of which should make it invaluable.

Lest the discussion of so simple a thing as the reaction of a jet of water might seem unnecessary, let me say to those to whom its action is, or seems to be, clear, that while it is simple when the general results only of the phenomenon are considered, it becomes complex when the details of the action of the individual particles are discussed, and this complexity is confusing to many, especially if it appears difficult to reconcile it with a simpler view of the action of the jet. I have, therefore, added a discussion, in a simple case of the distribution of pressures in a vessel of flowing water, for the purpose of showing the agreement of the results thus derived with those obtained from a more general standpoint.

Of course the simplest way to arrive at the reaction of a fluid jet is to seek that constant force which, acting during one second, will give the velocity of the jet to the quantity of fluid which enters it per second. This force equals the product of the mass of the

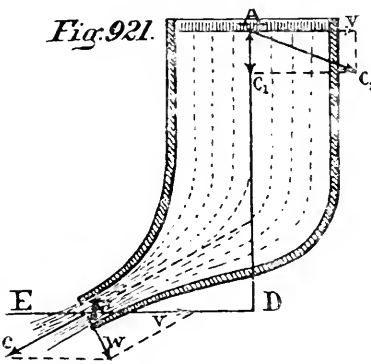
fluid into the jet velocity; the reaction of a fluid jet is therefore equal to the momentum per second of the jet.

As there is to be no discussion of the effect of friction, the otherwise useful distinction, due to Rankine, between *direct action* and *reaction*, will not be made.

I have translated the paragraphs themselves from the last German edition. In § 522 the original has been closely followed, while, in the next section, the language has been shortened somewhat, without in any way altering the sense. The equations have also been numbered, and with odd numbers alone, to allow corresponding even numbers to be used in the comments upon them.

No apology is needed for the notation of this article. To make it more systematic would evidently involve a partial change of Weisbach's notation, which, though it may be advisable in a revision of the original, would not contribute to the value of a review.

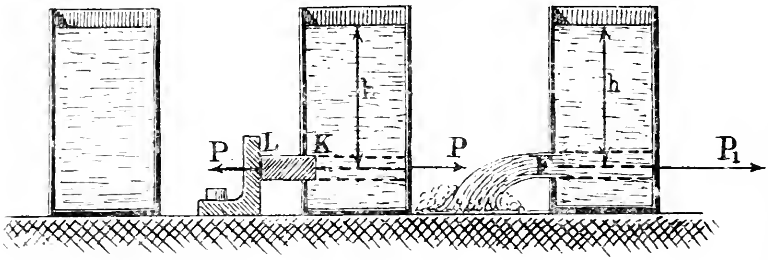
Fig. 921.



"§ 522. REACTION OF WATER.—In a vessel of still water the total pressure of the water against the vessel reduces itself to a vertical force equal to the weight of the water; when, however, the vessel $A F$, *Fig 921*, has an opening, F, through which the water flows out, this pressure is changed, not only because at F a portion of the wall of the vessel is wanting, but also because the water, as it flows toward the orifice, reacts by virtue of its inertia, as every body does whose condition of motion is being changed. As this change may include an alteration of velocity as well as of direction of motion, so may the *reaction* of the out-flowing water arise as well from an acceleration as from a change in direction of the water flowing toward the opening."

Weisbach has already, in a previous paragraph, made a plain statement as to the reaction of a jet. The pressure of the vessel of still water downward upon its support equals the weight of the water (see *Fig. 1*), the vessel itself being supposed without weight, as

Fig. 1. Fig. 688.



will be done throughout this discussion. In *Fig. 688*, the effect of the removal of a portion of the wall of the vessel is illustrated; the left-hand portion of the figure shows the opening filled with a frictionless piston $K\,L$, which abuts against the fixed angle-block, L, and sustains a pressure from the water $= P = F h \gamma$, where F is the area of the opening, h the head of water in feet and γ the weight of a cubic foot: now, while in *Fig. 1* the horizontal pressures of the water on the vessel balance each other, in this figure the pressure on the right-hand wall will be greater than that on the left by the amount sustained by the piston and block, therefore the water exerts a force, tending to move the vessel to the right, equal to

$$P = F h \gamma. \tag{1}$$

In the right-hand portion of the figure, the vessel has been moved to the right, so as to allow the water to flow out, and the statement is made that the reaction of the out-flowing water adds an equal amount and increases, therefore, the force tending to move the vessel to the right to

$$P_1 = 2 F h \gamma; \tag{3}$$

but proof of this is deferred, nor is the effect upon the weight of the water alluded to.

The term "inertia" were better omitted in the science of mechanics as being of no scientific value. Except so far as it is synonymous with "mass," it attaches to no definite or useful idea other than the statement that for all change of motion there must

be a cause, but, as all science is based upon the hypothesis that nothing occurs without a cause, such a statement would seem superfluous. If it be supposed that "inertia" refers to a tendency of matter to oppose itself to being moved, or having its motion changed, or to a laziness or inability to set itself in motion, then the term is not only useless but misleading, for two particles of matter left to themselves in each other's presence immediately begin to move toward each other.

The term "reaction" generally applied to these phenomena certainly does not in all cases accurately describe them. When no water flows out, the weight of each particle is transmitted unchanged to the bottom of the vessel as a downward pressure; in the other case, the particles have their motion changed and accelerated as they approach the orifice, and therefore some of them cause a greater and some a less pressure on the bottom than that due to their weights. Viewing the phenomenon in another way, an orifice in a vessel of water allows the atmospheric pressure to penetrate, so to speak, through the opening and produce an area of low pressure in the neighborhood thereof, and it is this low pressure that causes the flow toward the orifice and a diminution around it of the pressure of the water against the vessel.

"In the following manner we arrive at a knowledge of the complete reaction of the out-flowing water."

Notice the word out-flowing (ausfließenden), which the American edition lacks, because it is a distinct statement of the object of the analysis which follows. The term "out-flowing" and the stronger (perhaps too strong) terms "issuing water"—"water which issues"—used in the American version of the preceding paragraph, must not be taken as referring to the water after it has arrived at, or passed out of, the orifice, inasmuch as the effect to be considered is that of the water approaching the same.

"Let c be the velocity of the water flowing through F, c_1 the relative velocity of the water at the surface at A, G the area of this surface and h the head $A\ D$. We shall then have

$$\frac{c^2}{2g} = h + \frac{c_1^{\,2}}{2g}, \qquad (5)$$

and for Q, the delivery per second

$$Q = F\,c = G\,c_1. \qquad (7)$$

Suppose now that the vessel $A F$ is moving horizontally with a velocity v, then the absolute velocity c_2 of the entering water will be

$$c_2^2 = c_1^2 + v^2, \tag{9}$$

and, putting α for the angle of depression $E F c$ of the jet-axis, we have for the absolute velocity w of the jet

$$w^2 = c^2 + v^2 - 2\, c\, v \cos \alpha. \tag{11}$$

Now the energy of the water before efflux is

$$L_1 = \left(\frac{c_2^2}{2g} + h\right) Q \gamma = \left(\frac{c_1^2 + v^2}{2g} + h\right) Q \gamma, \tag{13}$$

but after efflux it is

$$L_2 = \frac{w^2}{2g} Q \gamma = \frac{c^2 + v^2 - 2\, c\, v \cos \alpha}{2g} Q \gamma; \tag{15}$$

it therefore follows that the amount of energy taken from the water and given to the vessel is

$$L = L_1 - L_2 = \left(\frac{c_1^2 - c^2 + 2\, c\, v \cos \alpha}{2g} + h\right) Q \gamma, \tag{17}$$

but $h = \frac{c^2}{2g} - \frac{c_1^2}{2g}$, therefore

$$L = \frac{c\, v \cos \alpha}{2g} Q \gamma; \tag{19}$$

from this we get for the *horizontal component of the reaction*,

$$H = \frac{L}{v} = \frac{c \cos \alpha}{g} Q \gamma. \tag{21}$$

If in this we substitute $Q = F c$, and then the value

$$\frac{c^2}{2g} = \frac{h}{1 - \left(\frac{F}{G}\right)^2}, \tag{23}$$

obtained from

$$\frac{c^2}{2g} = h + \frac{c_1^2}{2g} = h + \left(\frac{F}{G}\right)^2 \frac{c^2}{2g},$$

we get

$$H = \frac{c \cos \alpha}{g} F c \gamma = 2 \frac{c^2}{2g} F \gamma \cos \alpha = 2 F \gamma \cos \alpha \frac{h}{1 - \left(\frac{F}{G}\right)^2}. \tag{25}$$

If F is small compared with G we get

$$H = 2\, h\, F \gamma \cos \alpha \tag{27}$$

and if the jet be horizontal

$$H = 2\ h\ F\ \gamma \tag{29}$$

Therefore, *the reaction of a horizontal jet is equal to the weight of a column of water, whose base equals the cross section of the jet, and whose height* (2 h) *is double that due to the velocity*."

In commenting upon the above analysis, it will be of advantage to emphasize the most important height in the problem (that due to the velocity of the jet) by giving it a symbol, h', of its own, so that equation (5) becomes

$$h' = \frac{c^2}{2g} = h + \frac{c_1^{\ 2}}{2\,g} \tag{6}$$

reducing (13) and (15) to

$$L_1 = \left(\frac{v^2}{2\,g} + h' \right) Q\,\gamma \tag{14}$$

and

$$L_2 = \left(\frac{v^2 - 2\ c\ v\ \cos\alpha}{2\,g} + h' \right) Q\,\gamma, \tag{16}$$

so that equation (19) comes at once by substraction. Equation (23) may now be written

$$h' = \frac{c^2}{2\,g} = \frac{h}{1 - \left(\frac{F}{G} \right)^2} \tag{24}$$

and regarded solely as an equation showing the relation between h' and h, not to be used, however, in producing (25), which is to be written

$$H = \frac{c\ \cos\alpha}{g}\,F\,c\,\gamma = 2\,\frac{c^2}{2\,g}\,F\,\gamma\,\cos\alpha = 2\ F\,h'\,\gamma\,\cos\alpha. \tag{26}$$

Putting $\alpha = o$, this reduces to

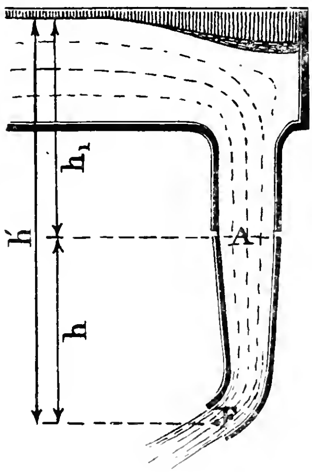
$$H = 2\ F\,h'\,\gamma, \tag{30}$$

Equations (26) and (30) are identical with (27) and (29) when h becomes $= h'$, *i. e.*, when $G = \infty$, but they have been obtained without any suppositions restricting F and G and are therefore general. In the final italicized statement of the proposition "(2 h)" must also be written (2h'), as will be seen later; in fact, the introduction of h', while advantageous in simplifying the analysis, has for its main object the removal of a serious ambiguity in the use of h.

This analysis commences with a statement of the relations

between the water and the vessel, both c and c_1 being, of course, velocities of the former with regard to the latter. Equation (5) expresses the conservation of energy on the supposition that none is lost in heat; or, otherwise, it states that the "total head," with reference to the horizontal plane E D, is the same at A and F. Equation (7) is the "equation of continuity," expressing the fact that as fast as the water flows out at F, with velocity c, the same quantity flows in through the cross section G at A, with velocity c_1. The "surface at A" must not be taken literally to mean that there is an upper or "free" surface to the fluid, for in that case there would be no means of keeping up the supply and h, c, and c_1

Fig. 2.



would gradually decrease as the water ran out. All the equations, however, suppose that there is a "steady flow," and therefore the surface at A is simply a cross section of the stream, having, however, the peculiarity that at this section the pressure $= p_1 = 14{\cdot}7$ pounds per square inch $=$ atmospheric pressure, the same as it would be on a free surface. To maintain the pressure p_1 constant the water must be supplied at A at exactly the speed with which it flows away from A and, to make sure that it is, the pipe should be cut apart at A to allow the atmosphere to have access to the stream; water coming out of the crack will indicate that it is being supplied faster than it can get away with the velocity c_1,

which is dependent upon h, G and F, as shown by equations (5) and (7); air going in will show the reverse. One means of furnishing the right amount is indicated in *Fig. 2*, where h_1 is the height due to the velocity c_1. The joint at A also makes the vessel A F, an independent piece, so that by supporting it suitably by scales, the effect of the flowing water could be experimented upon.

The analysis now assumes that any uniform motion given to the vessel cannot affect the flow of the water; of course the supply arrangements must go along at the same velocity, the cut in the pipe, however, serves to keep the vessel free from all other parts. If the vessel is still with reference to the earth the relations between the kinetic energies with reference to the same are given by eq. (5), or (6), when, however, it moves the new velocities, eqs. (9) and (11), and from them the new energies, eqs. (13) and (15), or (14) and (16), must be calculated with respect to the earth.

The argument now is that, as to the motion of the water with reference to the vessel, eq. (5), or (6), shows that no energy can disappear by the simple action of the head h in increasing c_1 to c; if, however, we consider the energy of the water with reference to the earth, the energy with which it enters the vessel, increased by its fall through the height h, should equal the energy of the issuing water, provided no force exists tending to move the vessel horizontally. If, however, there be a force H tending to increase the velocity v, then Hv, the work done by this force per second, must equal the energy which disappears per second from the water in flowing through the vessel. This energy, spoken of as being "given to the vessel," must be immediately absorbed in some way, so as to cause no change in v; we may, in fact, suppose the jet to be propelling a car or ship, and the energy to pass away in frictional and other resistances.

We will now consider the ambiguity in the use of h. Even without the specific introduction of h' eq. (25) says clearly that H depends upon c and α and, as shown before, eqs. (26) and (30), the demonstration requires only the introduction of h' to be complete, but if the last expression in (25), involving h and $F \div G$, be accepted as the value of H, some means must be taken to get rid of $F \div G$. Now, the supposition that "F is small compared with G," is somewhat misleading; it will hardly do to reduce the jet to nothing in attempting to find its action, because there is still another F in the expression, which is to be retained, and further,

because there is no necessity for the reduction. What we must suppose intended is to place $G = \infty$; that is, we may suppose the cross section of the supply to be enlarged until the velocity c_1 becomes 0, then any effect due to this velocity will have disappeared and we shall arrive at the action of the out-flowing water alone, without having altered the size of the jet. In this way the analysis as it stands is strictly correct, providing that the italicized paragraph be understood to be limited to the case $G = \infty$ (or, approximately, G very large), under which supposition it has been arrived at and for which only $h = h'$. This paragraph, as before stated, becomes true for all values of F and G by the substitution of $(2\,h')$ for $(2\,h)$, but in this case it follows directly from (25), or (26), any supposition regarding F or G being out of place. The supposition $G = \infty$ is also to be avoided because of the ∞ horizontal velocity required to bring distant water to the neighborhood of F, which for some minds would weaken the argument. The fact is that eq. (25) shows plainly, in spite of the introduction into it of F and G, that the entering velocity has nothing to do with H; the final equation and statement should, therefore, be based directly upon equation (25), or (26).

In the American edition the fraction $F \div G$ is not introduced and consequently no condition is imposed as to its value, but a result is obtained by substituting h for $c^2 \div 2\,g$, the error of which will be again referred to in considering the next section.

"§ 523. Suppose now, *Fig. 923*, the vessel's velocity v to be vertically upward, the absolute velocities of the in- and out-flowing water will be respectively

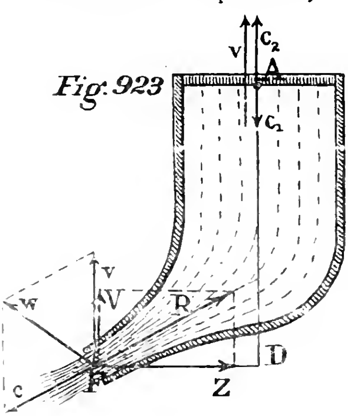


Fig. 923

$$c_2 = v - c_1 \qquad (31)$$

and

$$w^2 = c^2 + v^2 + 2\,c\,v\,cos\,(90^0 + \alpha)$$
$$= c^2 + v^2 - 2\,c\,v\,sin\,\alpha; \qquad (33)$$

hence the total energies of the water per second are respectively

$$L_1 = \left(\frac{(v - c_1)^2}{2\,g} + h\right) Q\,\gamma \qquad (35)$$

and

$$L_2 = \frac{c^2 + v^2 - 2\,c\,v\,sin\,\alpha}{2\,g}\,Q\,\gamma; \qquad (37)$$

consequently the work transferred to the vessel is

$$L = L_1 - L_2 = \left(\frac{c_1^2 - 2\,v\,c_1 - c^2 + 2\,c\,v\,sin\,\alpha}{2\,g} + h\right) Q\,\gamma, \quad (39)$$

which by (5) reduces to

$$L = \frac{(c\,sin\,\alpha - c_1)\,v}{g}\,Q\,\gamma, \quad (41)$$

and the corresponding vertical force is

$$V = \frac{L}{v} = \frac{c\,sin\,\alpha - c_1}{g}\,Q\,\gamma = \left(sin\,\alpha - \frac{F}{G}\right)\frac{c}{g}\,Q\,\gamma$$

$$= \left(sin\,a - \frac{F}{G}\right)\frac{c^2}{g}\,F\,\gamma = \left(sin\,\alpha - \frac{F}{G}\right) 2\,h\,F\,\gamma. \quad (43)$$

If F is small compared with G, we have $F \div G = 0$ and therefore the *vertical component of the reaction*

$$V = 2\,h\,F\,\gamma\,sin\,\alpha \quad (45)$$

which, combined with eq. (27), gives for the *complete reaction of the water,*

$$R = \sqrt{V^2 + H^2} = 2\,h\,F\,\gamma \quad (47)$$

in a direction exactly opposite to that of the jet."

It should be noted here that this is the end of the analysis proper, which consists in determining the horizontal and vertical reactions and then combining them into the resultant reaction; what follows consists of applications to particular values of α and $F \div G$. It should also be remarked that the supposition already made as regards the latter, which, as shown, is essentially a supposition as regards G alone ($G = \infty$) is simply a device for eliminating the effect of the entering water, while the suppositions which follow are to show the application of the result to special cases.

The introduction of h' gives in place of (35) and (37)

$$L_1 = \left(\frac{v^2 - 2\,c_1\,v}{2\,g} + h'\right) Q\,\gamma, \quad (36)$$

and

$$L_2 = \left(\frac{v^2 - 2\,c\,v\,sin\,\alpha}{2\,g} + h'\right) Q\,\gamma; \quad (38)$$

from which we get at once (41).

The last expression in (43) is evidently in error, inasmuch as h

has replaced $\frac{c^2}{g}$ in the preceding value, to which it is not equal until $F \div G$ has been made equal to zero; but this is probably accidental and has no effect on what follows. This last value for the vertical force should therefore be

$$V = 2\,F\,\gamma\left(\sin\alpha - \frac{F}{G}\right)\frac{h}{1-\left(\frac{F}{G}\right)^2}$$

which corresponds in form with the final value of (25).

By the introduction of h' (43) takes the form

$$V = \frac{c\sin\alpha - c_1}{g}\,Q\,\gamma = \left(\sin\alpha - \frac{F}{G}\right)\frac{c^2}{g}\,F\,\gamma = 2\,F\,h'\,\gamma\left(\sin\alpha - \frac{F}{G}\right) \tag{44}$$

Having pointed out the distinction between h and h' and the fact that the reaction of the jet depends upon the latter we will now call attention to a similar distinction to be made as regards the different V's. It will be noticed that V is not called the *vertical reaction* previous to eq. (45), and it becomes so then only because $G = \infty$. V in (43) and (44) includes the vertical reaction of the entering water; the weight of the water in the vessel is not included and will be referred to later. Writing again the values for H and V, from (21) and (43) it will be evident that they contain a full solution of the problem.

Evidently they may be thus written;

$$H = Q\frac{\gamma}{g}\cdot c\cos\alpha \tag{42}$$

and

$$V = Q\frac{\gamma}{g}\cdot(c\sin\alpha - c_1), \tag{44}$$

where $Q\frac{\gamma}{g} =$ mass of water flowing per second, $c\cos\alpha =$ the horizontal component of the velocity of the jet, and $c\sin\alpha - c_1 =$ the vertical component of the same less the vertical component ($=$ whole velocity) of the entering water. We have, therefore, $H =$ horizontal momentum of jet, $V =$ vertical momentum of out-flowing jet plus the vertical momentum of in-flowing jet, which latter is negative.

The fact that V is the sum of the actions of both out- and in-flowing water is evident also from the fact that the value of V consists of the sum of two terms, one of which depends entirely upon the entering velocity and the other upon the vertical component of the velocity of the jet. If $\alpha = 0$ V depends entirely upon c_1 and the weight of the vessel of water will be apparently increased by the momentum of the in-flowing jet only, but for a downward jet ($\alpha = 90°$) V depends on the difference of velocities, *i. e.*, upon the increase of velocity as the water passes through the vessel. Comparing also the two equations we see that the action of each jet is in a line with its axis, the in-flowing jet having no horizontal reaction and the out-flowing one having the regular form for its horizontal and vertical reactions that any force at an angle α would have. We are therefore justified in separating these parts of V and writing

$$V' = Q \frac{\gamma}{g} c \sin \alpha \qquad (19)$$

and

$$R = \sqrt{H^2 + V'^2} = Q \frac{\gamma}{g} c$$

where H, V' and R are the horizontal, vertical and oblique (or total) reactions of the out-flowing jet only, and equal its horizontal, vertical and total momenta, which was the problem originally proposed.

With the introduction of h', these values become

$$H = 2\,F h' \gamma \cos \alpha \qquad (26)$$

$$V' = 2\,F h' \gamma \sin \alpha \qquad (46)$$

$$R = 2\,F h' \gamma \qquad (48)$$

In *Fig. 923*, the horizontal reaction is marked Z; the vertical force drawn, and marked V, is evidently V' because the resultant of it and Z is drawn in the axis of the jet.

In the American edition the following errors are to be noted in the above analysis: In (35) c appears instead of c_1, in (37) γ is omitted and in (39) the minus sign of $2\,v\,c_1$ is lacking.

We come now to that part of the analysis devoted to the consideration of special cases.

"If $F = G$, that is, if the water flows through a uniform pipe, $F \div G = 1$ and therefore,

$$V = (\sin \alpha - 1)\,2\,F h \gamma = -(1 - \sin \alpha)\,2\,F h \gamma \qquad (49)$$

and V does not act upward, but downward.

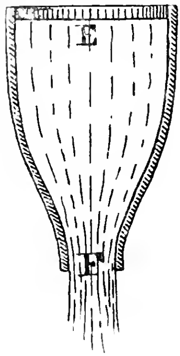
For $\alpha = -90°$, that is, when the pipe has a semi-circular bend, we have

$$H = 0 \; and \; V = R = -\left(1 + \frac{F}{G}\right) 2\, F h \gamma, \tag{51}$$

which last value becomes, for $F = G$,

$$V = R = -4\, F h \gamma \tag{53}$$

Fig. 924.



If $\alpha = +90°$, a condition represented by *Fig. 924*, we have

$$H = 0 \text{ and } V = R = \left(1 - \frac{F}{G}\right) 2\, F h \gamma \tag{55}$$

and, consequently, for $F \div G = 0$,

$$V = R = 2\, F h \gamma. \tag{57}$$

The total weight of the water in the vessel will be diminished by this amount when the water is allowed to flow."

We have here three cases considered: the general case $F = G$; the case $\alpha = -90°$, with the sub-case $F = G$; and the case $\alpha = +90°$, with the sub-case, $F \div G = 0$. It should be noticed that when $F = G$ it is not to the action of the out-flowing jet alone that the equations refer, for they give the values of V, and not of V', and to this extent they are an extension of, or a digression from, the original problem. The analysis is, however, the more elegant, inasmuch as it covers the action of both the out-flowing and in-flowing jets, and gives, therefore, their effect upon the weight of the water.

(*To be continued.*)

BOOK NOTICES.

Die Schiffsmaschine, ihre Construction, Wirkungsweise und Bedienung. Mit einem Atlas lithographirter Tafeln, bearbeitet von Carl Busley, kaiserlicher Marine-Ingenieur, Lehrer an der kaiserlichen Marine-Akademie und Schule in Kiel, etc. Kiel. Lipsius & Fischer. 1885.

This excellent book, which has been worked up with true German thoroughness, supplies a long-felt want among German engineers, ship-builders, naval officers, and others interested in shipping, who have been, during a long struggle to develop German naval and mercantile ship and marine engine construction, dependent on foreign (mainly French and English) literature for information. Considering how rapid has been the march of progress in the last twenty years, it is no small compliment to the author to say that his work is not only up to the latest improvement, but that he has produced a book whose equal is not to be found in any other language, either for the complete theoretical treatment of all subjects connected with steam-shipping, or for the mass of information on the latest practice, which is given by the extensive collection of drawings and their description.

The author starts from first principles, by stating the laws of matter, motion, energy, etc., briefly; then going into the mechanical theory of heat, its application to gases, and especially to steam. Combustion and combustibles are next taken up, followed by theories on performance, proportions and power of marine engines, coal consumption, economy, efficiency, proper rates of expansion, speed and efficiency of steamships, etc.

Coming now to practice, we find a description of steam boilers, giving types, material, construction, furnaces, mountings, lagging, arrangements of fire-rooms, bunkers, etc.; in fact, everything that is necessary to get the machinery ready for service. A section on management of boilers—their behavior when in service, corrosion, incrustation, etc.—winds up with a chapter on explosions, which is principally remarkable for refuting some of those extraordinary theories formerly current on that subject, and adopting a common-sense view.

The next chapter is devoted to laws and regulations governing construction and management of steam boilers in the German Empire, and also those adopted by the German Admiralty.

The next chapter treats fully of all auxiliary machinery on board modern merchant or naval vessels, such as turret-turning engines, air compressors, windlasses, steering engines, winders, electric light plant, etc.

The next chapters come to the marine engine itself, with all its interesting details, describing the different types up to the triple-expansion system, which is but briefly alluded to in a doubting manner, which the author, in the light of the latest developments, would now, perhaps, be ready to modify. A very complete description of valve gears and diagrams closes the first volume.

In the second volume, after going fully into all other details of the marine engine, pumps, injectors, etc., the equipment of engines (lubrication, instruments, lamps, etc., erection and completion, pipes, valves, etc.) is described,

weights of all parts are given, and several examples of complete specifications. The indicator and its application, management of marine machinery, repairs, trial trips, etc., all these with special application to naval vessels, are fully treated. Spare parts, tools, utensils, stoves, material for repairs, etc., come next, while finally the propulsion of vessels is extensively gone into, first with regard to laws of resistance, then the construction of propellers, including paddle wheels, screws, hydraulic wheels, etc.

Vol. iii contains, on 170 plates, about 1,200 lithographed drawings, all made to scale from working drawings, and executed in the best manner. This part of the book alone is a valuable record of the gradual progress and present practice of all details in marine engine construction and its adjuncts.

It may safely be predicted that a translation of this valuable work into English, would be readily disposed of in England and the United States.

Philadelphia, June 1, 1887. JOHN HAUG.

LEVELLING. By Prof. Ira O. Baker. Van Nostrand's Science Series.

This little work is eminently practical in the true sense of the word. It may be called a criticism, too, on levelling, since it points out the defects in particular of each process. Thus it gives to the student a correct appreciation of the barometer, the vertical circle and the spirit level. We are pleased to see a work of this character appear. Writers of more pretentious volumes labor often under the impression that a plea needs to be made for art and science. Hence they bring forward abnormally good results and remarkably close agreements as examples. Most readers, unfortunately, take them as *current* practice, and afterward become disappointed by personal experience. Thus arises much of the talk about the conflict of theory and practice. We recommend Prof. Baker's essay also for the clearness and conciseness with which it is written. It contains much serviceable matter to the student and to the practitioner, well expressed. In discussing precise spirit-levelling, the author seems to dwell upon the coast survey practice in preference to the European; but we prophecy that the labor and complication of the former will cause it to be abandoned in this country. E.

SCIENTIFIC NOTES AND COMMENTS.

ASTRONOMY AND PHYSICS.

A NEW METEOROLOGICAL OBSERVATORY.—A. Lawrence Rotch has recently published the *Results of the Meteorological Observations made at Blue Hill Meteorological Observatory, Massachusetts, in the Year 1886*, under his own direction. Great Blue Hill, on which the observatory is located, is 635 feet above the sea level, and is not only the highest land in Eastern Massachusetts (about ten miles out from Boston), but is also the highest point within ten miles of the Atlantic Coast from Maine to Florida. The observatory was erected at a cost of about $3,500, and the current yearly expenses of about

$2,500 are borne by the director. Considerable interest attaches to this new enterprise, from the fact that it is now not only probably the best equipped meteorological observatory on this Continent, but because the observations are, as they should be, made for the most part with automatically recording instruments, with occasional checking by direct eye readings. If the science of meteorology is ever successfully to refute the charge of publishing tome upon tome of disconnected and aimless observations, it must be through observatories that furnish *continuous* records of *all* the weather factors. Although the scope of Mr. Rotch's observatory is wider in this respect than that of any American institution we know of, it is hoped this is but a beginning in an attempt to automatically record and observe every known factor. It is gratifying to note the character of the equipment. It consists of a standard barometer by Hicks, compared at Kew, and one by Green; a Draper self-recording mercurial barometer and an aneroid barograph by the Richard Brothers, Paris; thermometric standards by Hicks and by Baudin, with working standards by H. J. Green, a Richard's thermograph; a Koppe hair hygrometer, wet and dry bulb thermometers; a Draper recording anemoscope, a Draper recording anemometer, a Robinson anemometer, with the Gibbon self-register; a Rotch wind-pressure gauge; Rotch self-recording rain and snow gauges; the Cambell-Stokes sunshine recorder, a Jordan sunshine recorder; a cloud mirror for the measurement of the azimuth and altitude of clouds, and other instruments. The results are printed in neat tabular form, and are accompanied by tracings from the self-recording instruments, to illustrate certain meteorological phenomena. M. B. S.

ON THE SCINTILLATION OF STARS.—K. Exner (*Astron. Nach.*, **116**, 106), in view of the present problem of determining places where large instruments would suffer least from atmospheric disturbances, draws attention to the necessity of there studying the scintillation of stars. It is well known that the larger the aperture of the instrument, the less unsteadiness is noticed in fixed stars, and this, as Newton long since remarked, arises from the fact that the image is an integration of numberless rapidly vibrating independent rays, and, is consequently, larger and as a whole relatively quiescent. To show the independent movements, there was placed before the twelve-inch refractor of the Vienna Observatory a cap with three small openings lying in a straight line. On directing to *Sirius* and pushing in the ocular, the three round images of the star appeared in continual relative motion. Observation, by means of an instrument of large aperture, therefore shows the stars as quiescent discs, or as circles of dispersion due to scintillation (*Scintillationszerstreuungskreise*) whose radii equal the amplitude of vibration, which the star would show with reduced aperture. Authorities are cited to show the numerical value of this amplitude, Holetschek finding it $5''$ to $7''\cdot5$, Struve, $4''$; Carlini oscillations of $10''$ to $12''$, while Dr. Exner, by scintillometer observations, determined the amplitude at $6''$. Scintillation is far greater by day than by night; Montigny measuring its amplitude by day as great as $25''$.

The advantage of scintillometer observations is considered by the author

the more evident, since measurements have never yet been made to determine the relation between the grade of scintillation and the elevation of the place of observation, and because he is convinced, from unpublished observations, that scintillation of the stars arises principally in the lower strata of the atmosphere. M. B. S.

ATMOSPHERIC ELECTRICITY.—R. Nohrwold (*Wied. Ann.*, **31**, 448), from a long series of experiments, draws some interesting conclusions as to the possibility of electrifying air. By coating a suitable glass receiver on the inside with glycerine, and then filling it with smoke, he is able to show to a considerable audience that one or two turns of a Töpler machine discharged from a point within the receiver, is sufficient to clear the air by projecting the smoke upon the glycerine. Following up the question of statically electrifying pure air by a great variety of tests, he concludes that he has shown more thoroughly than has been done heretofore, that electricity, streaming out from points, cannot statically electrify the air itself, but rather the suspended dust consisting of solid or liquid bodies. Glowing platinum wire was found to render pure air capable of being electrified, on account of the slow evaporation or emission of small particles, and it was shown that on these particles, and not in the air, the charge resided. He concludes that it is very probable that atmospheric air and other gases probably conduct themselves similarly—cannot be statically electrified. He also gives a new experiment, according to which, at ordinary temperatures, negative electricity of high potential more readily escapes from solid conductors into atmospheric air, than positive electricity. M. B. S.

FINE THREADS OF QUARTZ.—C. V. Boys (*Phil. Mag.*, **23**, 489), gives an account of some very interesting experiments in the production of the finest threads of glass and other materials, with their properties and some suggested uses. The most remarkable threads he has found are those of quartz. Of these, he says: "As torsion threads, these fibres of quartz would seem to be more perfect in their elasticity than any known; they are as strong as steel, and can be made of any reasonable length, perfectly uniform in diameter, and, as already explained, exceedingly fine (*i. e.*, 'beyond the power of any possible microscope'). The tail ends of those that become invisible must have a moment of torsion 100,000,000 times less than ordinary spun glass; and, though it is impossible to manipulate with those, there is no difficulty with threads less than one-ten-thousandth of an inch in diameter." The drawing is neatly accomplished by using a small cross bow and very light arrow. One end of the glass is attached to the arrow, while the inertia of even a very small mass is sufficient to prevent the other end from following. Since these fibres can, by this method, be made finer than any cobweb, and also, in the case of quartz, possess the remarkable elasticity indicated, a variety of interesting applications may be expected. M. B. S.

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THE CHEMICAL BASIS OF PLANT FORMS.

BY HELEN C. DE S. ABBOTT.

[*A Lecture delivered before the* FRANKLIN INSTITUTE, *January 24, 1887.*]

The boundary between the mineral and vegetable kingdom is not a definite line. The individual of the one encroaches upon the dominion of the other. The terms "non-living" and "living matter" are only relatively accurate. [1]"Nature in all its manifestations constitutes a unity," * * * and "all matter is in a sense living."

Through chemical evolution a condition of matter obtains favorable for functional activity or life. This state may be described [2]"as a colloidal albuminoid united with more or less water." Its

[1] *Mineral Physiology and Physiography.* By T. Sterry Hunt. Boston, 1886, p. 18.

[2] *Ibid.*

simplest expression is found in the low forms of plants, slime-mould for example. This colloidal basis of life is protoplasm, a chemical compound of complex constitution, very unstable, and manifesting when alive certain properties called vital or "biotic."[1]

Active chemical changes are inseparably associated with both living and dead protoplasm. Synthetical or progressive processes prevail in life, analytical or retrogressive in death.

Absorption, metabolism, excretion, reproduction, contractility, automatism, and irritability are the properties of living matter; disorganization and dissociation those of dead matter.

Chemists have to discover the subtle differences between the chemical equation of living and dead protoplasm.

I wish to speak of some of the chemical compounds of plants, or more properly the chemical forms, since the structure of all plants is built up of chemical constituents. This subject is as extensive as the genera and species of the vegetable kingdom.

Last August,[2] I read a paper on certain chemical constituents of plants considered in relation to their morphology and evolution. The facts cited tended to show a chemical progression in plants, and a mutual dependence between chemical constituents and change of form.

Among the conclusions reached, were the following:

(1.) A similarity of one or more chemical constituents is to be found in all plants which are equally developed, and on the same evolutionary plane.

(2.) The evolution of chemical constituents follows parallel lines with the evolutionary course of plant forms, the one being intimately connected with the other, and consequently chemical constituents are indicative of the height of the scale of progression, and are essentially appropriate for a basis of botanical classification; in other words, the theory of evolution in plant life is best illustrated by the chemical constituents of vegetable forms.

Chemistry will aid us to comprehend the laws of evolution controlling plant forms. Evolution should also apply to chemical compounds as well as to morphology, since the latter can be shown to depend upon chemistry in general.

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[1] Dr. T. Sterry Hunt.

[2] Chem. Section Am. Assoc. for the Adv. of Science. Buffalo, 1886. Abstract, *Bot. Gazette*, *xi*, No. 10, Oct., 1886.

We have no certain knowledge of the precise chemical changes which take place in transforming carbon, hydrogen, oxygen, nitrogen, sulphur and other elements into the starches and proteids. We know, however, certainly the necessary conditions for many of these changes. The law controlling the absolute relation or the connective link between the form of a plant and its chemical composition is undetermined. But investigations in plant chemistry have not been conducted with this end in view. The facts which I have to offer to sustain the theory of a possible relation between plant forms and chemical compounds may seem to some inadequate, but no other explanation than the one offered to account for these statements has been suggested.

The chemical composition of the cell contents and wall has been determined in many plants; also of their roots, leaves, flowers and fruits.

Most of the ash constituents essential or injurious to the growth and development of plants are known, and also the variations in growth caused by the presence or absence of certain inorganic compounds.

The chemical changes through which many plants pass from the germination of their seed to maturity and decay are also known, each separate stage of growth showing a distinct chemical composition or a predominance of some one chemical compound.

It should be especially noted that some chemical compounds occur in certain species of plants and do not occur in others. Certain classes of compounds are found widely distributed through the plant kingdom, accompanied by correlated morphological characters. Some one compound, as saponin, will be found with similar botanical characters in plants of distinct genera and families, on the same plane of evolution or development.

It cannot be the result of accident, that cinchona and related plants contain quinine; and other plants distributed through the vegetable kingdom their own typical compounds. Nor can it be the result of accident or changes produced by climate or other causes, that an absence of some one or more compounds is accompanied by a modification of the exterior of the plant.

Before taking up the consideration of the above statements in detail, it may be well to study briefly two properties[1] of living protoplasm, namely, absorption and metabolism.

[1] *Lectures on the Physiology of Plants.* By S. H. Vines. Cambridge, 1886.

The seeds of plants are the store-houses of a certain amount of latent energy or life, elaborated by the parent plant and stored up in the form of complex chemical compounds. Under suitable conditions of warmth and moisture, certain chemical changes take place within the seed. The latent energy becomes active, and the seedling grows, feeding upon its food supply until it has exhausted its store.

At this stage the little plant must seek from without its food, from the atmosphere and the soil. The soil is of varying and complex composition, containing between its particles gases and moisture. The air which surrounds the leaves of land plants is a mixture of nitrogen and oxygen with small quantities of carbon-dioxide, ammonia, varying quantities of aqueous vapor and occasionally traces of nitric acid.

The elements from these media are absorbed by different parts of the plant, and there is a difference in the manner of absorption by fungi, parasites, air plants, and green plants. However, the elements which are absolutely essential for the nutrition and maintenance of the life of all plants are carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, potassium, calcium, magnesium, iron in the case of green plants, and in certain cases chlorine.

It is characteristic of plants that they must absorb their food in the fluid form. The absorbent organs of plants are the roots for water and salts in solution, and the leaves for gases. In the lower plants, where there are no roots nor leaves, water or substances in solution and gases are absorbed, either directly by the cells of the Thallus or by root hairs. Among the higher plants, the root hairs and the uncuticularized epidermal cells of the younger roots are active in absorbing material from the soil. Any part of the plant, if immersed in water, will absorb a smaller or a larger quantity of it; as, for instance, cut flowers placed with their cut ends in water will absorb, for a time, sufficient to prevent withering. The absorption of gases in higher plants is by means of the leaves, for it has been found that carbon-dioxide is absorbed from the air by those organs which are green and contain chlorophyl, and in experiments where the carbon-dioxide of the air was cut off from the leaves, though it was supplied to the roots, it was found that the plant could not live long. It has also been found that the presence of carbon-dioxide in another part of the plant does not contribute to the formation of starch in the leaves.

Green plants obtain their carbon from the carbon-dioxide of the air. Plants which do not contain chlorophyl, obtain their carbon by the absorption of complex organic substances. Green plants can absorb complex carbon compounds, and it has been proved, by direct experiment, that they can take up these complex substances when supplied to their roots.

Darwin[1] has shown that the insectivorous plants, by means of their modified leaves, absorb complex compounds, and that these are of importance in their nutrition. Flies and other small insects may often be found clasped in the tentacles of the *Drosera*, and in those experiments small pieces of meat, when placed on the leaves, were dissolved after a time by the secretions of the leaf glands and absorbed.

Hydrogen is absorbed by all plants in combination in the form of water or ammonia and its compounds, or in the complex substances mentioned above.

Oxygen is taken up by plants free or in combination in water or in salts. The free oxygen is especially concerned in destructive metabolic processes. The large quantities of this gas absorbed by plants, and especially by Fungi, show conclusively its consumption in metabolic processes.

The process known as the respiration of plants is the absorption of oxygen and the exhalation of carbon-dioxide.

The researches of Garreau[2] show that two distinct processes are in operation when leaves are exposed to the light, in the one oxygen is absorbed and carbon-dioxide is exhaled; in the other, carbon-dioxide is absorbed and oxygen is exhaled. When the leaves are exposed to a very bright sunlight, carbon-dioxide is absorbed and oxygen is exhaled, and the activity of these processes is so much greater than the absorption of oxygen and the exhalation of carbon-dioxide, that it appears as if the former only were in operation.

Gases, like solids, can only be assimilated in solution, and as they are soluble in water, the cell-walls of submerged plants may absorb them, and the sap near the surface of land plants will dissolve the gases from the atmosphere. The sap of plants contains in

[1] *Insectivorous Plants.*

[2] *Ann. d. Sci. Nat.*, Ser. 3, t. [illegible].

solution carbon-dioxide, oxygen, and also a certain amount of free nitrogen. That this nitrogen does not enter into the metabolism of the plant seems completely decided by the experiments[1] of Lawes, Gilbert, and Pugh; but the more recent experiments of Atwater[2] and Hellriegel[3] should be compared in this connection, and the matter cannot be said to be definitely settled.

I can only enumerate in this connection, without going into the subject, the possible sources[4] of the nitrogen supply:

(1.) Organic, nitrogenous matter.

(2.) The ammonia of the air, and of the ocean.

(3.) The nitrous and nitric compounds formed by combustion and by electric discharges.

(4.) Nitrogen fixed in the soil by microbes.

(5.) The free nitrogen of the atmosphere.

(6.) Mineral nitrates.

The sap which is continually flowing through living plants, is a watery fluid, holding in solution mineral matters, gases, and organic substances. The root hairs of plants penetrate the particles of soil and absorb the moisture from a film which surrounds each particle known as hygroscopic water. In thallophytes, the absorption is effected by the cells of the Thallus, and in Orchids a membrane invests the air roots especially adapted for the purpose. The distribution of water takes place, at least to some extent as its absorption, it passes by osmosis from cell to cell, as it passes originally from without into the superficial cells of the plant. The direction of this movement is not necessarily constant. The proportion of water in each cell varies, and the tendency to establish a fluid equilibrium will cause a current towards those tissues which are deficient. These statements apply equally to gases and other substances held in solution, which are needed for the continuance of the chemical and physical changes going on in the living cells of different parts of the plant.

The changes are more active for different substances in different

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[1] *Phil. Trans.*, 1860.

[2] *Amer. Chem. Jour.*, *viii*, Nos. 5 and 6.

[3] *Zeit. d. Ver. f. d. Rübenzucker Industrie*, Nov., 1886.

[4] "The Economical Aspect of Agr. Chemistry." By H. W. Wiley. *Proc. A. A. A. S.*, *xxxv*, 1886.

parts of the plant. The mineral substances absorbed by the roots pass up to the leaves, where they are concerned in the constructive metabolism going on in those organs. The products of these processes pass from the leaves to parts of the plant which are actively growing, and where plastic material is required, or to the seeds or other organs in which organic stores are being laid up.

If the stems or plants are cut in the spring, a flow of sap proceeds from the cut surface of that portion of the stem which is connected with the roots. This fact was investigated by Hales;[1] he concluded that there is "a considerable energy in the root to push up sap in the bleeding season." This force is termed the root pressure, and is the measure of the absorbent activity of the root hairs. The root pressure is not only manifested by causing the flow of sap; it also may cause the exudation of drops of sap on the surface. There is a marked periodicity in the flow of sap, which is not due to the immediate result of variations in external conditions, but is inherent in the absorbent cells themselves.

The current travels from the roots to the leaves through the lignified cell walls of the wood of the plant. The activity of the exhalation of watery vapor from the plant is not the same from their surfaces. The refreshing effect of a shower on withered leaves is due to the moisture penetrating the soil and being absorbed by the root hairs. From experiments[2] it has been shown that if the air is very moist and the leaves dry, that the leaf surfaces may absorb a little water.

The cuticle offers a certain amount of resistance to the passage through it of vapor which is due to resinous or waxy substances contained in it. The Mexican *Ocotilla*[3] offers a striking example. It grows in very dry and exposed parts of the country, where rainfalls are infrequent. The bark is chiefly composed of wax and resinous substances.

Other substances, as well as water, can be absorbed by leaves,[4]

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[1] *Statical Essays*, I, 1769 (4th Edition).

[2] Detmer and Boussingault.

[3] H. C. De S. Abbott, *Proc. A. A. A S.*, *xxxiii.* (*Am. Journal Pharm.*, Feb., 1885.)

[4] Boussingault; *Ann. Chem. et Phys.*, sér. V, *xiii*; also, *Agronomie*, VI, 1878. Mayer; *Landwirthschaftl. Versuchs-Stat.*, *xvii*, 1874.

experiments having shown that if a drop of calcium sulphate solution be placed on a leaf, it will have disappeared in the course of a few hours. This is more rapid when placed on the under surface. Though it seems that leaves may absorb water and substances in solution under certain circumstances, the especial absorptive function of leaves is the absorption of gases, as has been already explained.

The subject of the ash constituents of plants is a very important one in this connection. The essential mineral constituents of plants have already been mentioned: silicon, fluorine, manganese, sodium, lithium, rubidium, cæsium, barium, aluminium, zinc, copper, titanium, iodine, and bromine have also been found among the ash ingredients of certain plants.

The method of absorption of soluble mineral salts has already been described. A solution of insoluble salts is brought about in a different way. A soil rich in organic matter is always charged with carbon-dioxide, and this gas is also given off by the roots of living plants. Water containing this gas is able to dissolve calcium carbonate and some silicates, which are insoluble in pure water. The presence of certain soluble salts in the soil brings about a decomposition and renders the insoluble salts more readily soluble. Finally, the insoluble salts are brought into solution by means of the acid sap which saturates the cell wall of the root hair. This acid is not carbonic acid, for its reddening of litmus paper is permanent.

It has been shown by experiment that the chemical elements are not universally absorbed by roots in their combinations in the soil.

The wide differences in the composition of the ashes of plants show that each plant is endowed with a specific absorbent capacity. It is upon this fact that the "rotation of crops" in farming depends. A gramineous plant[1] is able to withdraw relatively larger quantities of silica from the soil than a leguminous plant. The latter can only do so to a very slight extent.

The absorbent capacities of nearly allied species are very different; again, individuals of the same species yield different ash compositions, depending upon their vigor; and the absorbent capacity of the plant varies at different periods of its life. It has

[1] Wolff; *Aschen Analysen*, 1871.

been stated that "similar kinds of plants, and especially the same parts of similar plants, exhibit a close general agreement in the composition of their ashes, while plants which are unlike in their botanical characters are also unlike in the proportions of their fixed ingredients."[1] If an ash constituent can pass through a cell wall, its absorption will take place independently of its use or harmfulness to the plant, but the absorption of essential inorganic constituents will depend upon its relation to the metabolism of the plant.

The ash constituents of a plant increase from the roots upwards to the leaves, a fact showing that the leaves are the organs in which more especially active chemical changes take place.

The ash ingredients are usually present in each plant cell; in the cell wall, imbedded in the cellulose, and partly in the contents of the cell. The salts of the alkaline metals and of the sulphates and the chlorides of magnesium and calcium occur in the solution of the sap. Silica and phosphates of calcium and magnesium are mostly insoluble and exist in the tissues of the plant.

Water culture experiments[2] have shown the essential ash ingredients. Potassium, like phosphorus, is always found in relation with living protoplasm. If[3] the plant was not supplied with potassium, it grew very little, and very little starch was formed in the chlorophyl corpuscles of the leaves. On the addition of potassium chloride, the starch grains became more numerous in the leaves, and made their appearance in other parts of the plants. Potassium, doubtless, plays an important rôle in the formation and the storing up of carbo-hydrates, for the organs in which these processes are active, as the leaves, seeds and tubers are found to be the richest in this element.

It has been observed that cæsium[4] and rubidium can replace potassium in the food of certain *Fungi* (mould, yeast and bacteria).

Salm-Horstmar describes[5] some experiments, from which he infers that minute traces of lithium and fluorine are indispensable

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[1] *How Crops Grow.* By S. W. Johnson. London, p. 145.

[2] Nobbe, Siegert, Wolff, Stohmann, Sachs, and others.

[3] Nobbe; *Die organische Leistung des Kaliums*, 1871.

[4] Naegeli; *Sitzber. d. Akad. d. Wiss. zu München*, 1880.

[5] *Jour. für Prakt. Chem.*, 1884, p. 140.

to the fruiting of barley. The same investigator has concluded that a trace of titanic acid is a necessary ingredient of plants.

Zinc[1] is also a frequent constituent of plants growing about zinc mines. Certain marked varieties of plants are peculiar to, and appear to have been developed by such soils, as the Violet, *var calaminarias* and Penny-cress. In the leaves of the latter plants, thirteen per cent. of zinc oxide was found ; in other plants, from ·3 per cent. to 3·3 per cent.

From the investigations of Baumann,[2] insoluble zinc salts in the soil are harmless to plants. All plants excepting the Coniferæ speedily die in a solution containing 10 mg. zinc to the litre, though traces of zinc in solution are harmless.

The specific action of zinc on the vegetable organism consists in a destruction of the chlorophyl coloring matter and a consequent stoppage of the whole process of assimilation.

Experiments[3] on maize, oats, buckwheat, show that arsenic attacks the protoplasm of the cell and destroys the power of osmose by the roots.

Sulphates occur in the cell sap of organs where chemical changes are rapidly taking place, and are doubtless formed in connection with the decomposition of proteids. Phosphorus occurs in actively growing cells in the most various plants. It has been found present in the green coloring matter of the leaves and is always found in relation with living protoplasm. Schumacher[4] holds, that the chief work of the alkaline phosphates is the acceleration of the diffusion of these difficultly diffusible albuminoids.

Calcium is especially abundant in the leaves of green trees and it cannot be replaced in the food of green leaves by any other metal. It can be replaced by strontium,[5] barium, or magnesium in the food of certain Fungi. Magnesium[6] resembles lime in many

[1] A. Braun and Risse (Sachs ; *Exp. Physiologie*, 153).

[2] *Landw. Versuchs-Stat.*, *xxxi*, 1-53 (*Jour. Chem. Soc.*, 1884, p. 1408.)

[3] F. Nobbe and others. *Landw. Versuchs-Stat.*, *xxx*, 381–422. (*Jour. Chem. Soc.*, 1884, p. 1409.)

[4] *Physik der Pflanze*, p. 128.

[5] Naegeli.

[6] R. Hornberger and E. V. Raumer ; *Bied. Centr. Bl.*, 1882, 837–844 (*Jour. Chem. Soc.*, 1883, p. 491.)

points, but is present in larger quantity in the stem and grain, and not in the leaves of the maize plant.

Iron is found to be essential to green plants only. If a seedling be cultivated by water culture in a fluid containing no iron, the leaves will become pale until at length they are nearly white, but on the addition of a small quantity of iron to the solution, or if the white leaves are painted with a dilute iron solution, they will very shortly become green. It plays an important part in the formation of the green coloring matter, though it does not enter into its chemical composition.

"Buckwheat,[1] barley and oats do not flourish when grown in solutions containing no chlorides, and as in these plants the chlorophyl corpuscles become overfilled with starch grains, it was thought that this element was of importance in connection with the translocation of carbo-hydrates."

Sodium[2] has been used in water culture to replace potassium, but the plants deprived of potash did not develop.

Manganese is abundant in the ash of *Trapa natans*. I also found it in the different portions of *Yucca angustifolia*.[3]

Iodine and bromine are found in marine *Algæ* and in minute quantity in some plants grown far from the sea.

Silica is found in the form of soluble or insoluble silicic acid. It occurs principally in the cell wall, but it has been found in the cell sap of a plant (*Equisetum hiemale*[4]), and certain cells in the pseudo bulbs of epiphytic Orchids[5] contain each a plate of silica.

Experiments have shown that the absorption of silicic[6] acid greatly assists the assimilation of other plant foods, and that plants to which it is supplied show a decidedly more healthy development of grain and straw than others not so treated. Silica is doubtless of mechanical use, giving firmness and rigidity to plant

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[1] Vines; Cambridge Edition, p. 136. Also, Beyer; *Landw. Versuchs-Stat.*, *xi*. Leydhecker; *ibid*, *viii*.

[2] Salm-Horstmar; Knop and Schreber.

[3] *Yucca Angustifolia*. Helen C. DeS. Abbott, *Trans. Am. Philos. Soc.*, Dec., 18, 1885.

[4] Lange; *Ber. d. deutsch. chem. Ges.*, *xi*.

[5] Pfitzer; *Flora*, 1877.

[6] C. Kreuzhage and E. Wolff; *Landw. Versuchs.-Stat.*, *xxx*, 161–198. (*Jour. Chem. Soc.*, 1884, p. 1112.)

tissues; though the real cause of "laying" of crops has been found to be due to the imperfect development of the tissue and not to an insufficient supply of silica.

The percentage of ash constituents in plants varies, but the quantity is sufficient to be a very important factor in the consideration of chemical forms of plants.

I have already said that the albuminous cell contents, called protoplasm, are always present in the living cells of plants. The introduction into the cell of the gases, water, and inorganic substances goes to the direct formation of this colloidal body, or assists in it.

It has been stated that the soil, water, and atmosphere supply the food of all plants. It would be of interest to dwell upon the processes of assimilation, and the chemical changes that go on within the living plant, if our time would allow. It may be mentioned that nowhere in any department of chemistry have our former views been more modified than in the physiological chemistry of the vegetable cell during the last three years.

For example, I may say, that, at least in some plants, nascent starch passes in a soluble form from cell to cell by osmosis without conversion into sugar, as was formerly held.

Sugar in some plants may be regarded as a waste product, resulting from the breaking down of more complex substances, of no further service in the development of the plant.

Sorghum[1] cane, at the time of the maturing of the seed and the full growth of the plant, contains the largest percentage of sugar, and this sugar appears to be really a waste product.

The classification[2] *of Plastic and Waste Products,* in Vines's late *Physiology*, cannot be accepted as final, since many changes in plant chemistry have resulted since 1882—the date of his chemical bibliography.

It may be generally said that the proteids or albuminoid substances are formed in the cell from a simple carbo-hydrate and some nitrogenous body, probably an amide.

The inorganic acids supply sulphur and other substances necessary to enter into combination with the proteid, or act mechanically by removing waste material.

[1] H. W. Wiley; *Botanical Gazette,* 1887.

[2] Cambridge Edition, 1886.

The function of chlorophyl may be briefly stated:[1] It absorbs certain rays of light, and thus enables the protoplasm of the cell to avail itself of the radiant energy of the sun's rays for the construction of organic substances from carbon-dioxide and water.

Plants which are grown in the dark, or at low temperature, are usually of a yellow color. Such plants are said to be etiolated. There is reason to think that this yellow substance, etiolin, is an intermediate substance in the formation of chlorophyl, for if it is exposed to light it is converted into a green color; these complex coloring matters are probably derivatives of protoplasm.

The autumnal change of leaves is owing to a third coloring matter, called xanthophyl; in many cases, the leaves also contain a red coloring matter, erythrophyl.

The importance of chlorophyl in the plant will be admitted when it is said that the absorption of carbon-dioxide, the evolution of oxygen, and the formation of many organic substances are effected solely by chlorophyl corpuscles.

The organic acids occur in plants free and also in combination with bases. It is to the presence of these bodies that the acid reaction of plant tissues is due. Some organic acids are assimilated by plants; the turgidity of cells is to be ascribed to their presence, and the acid sap in root hairs renders possible the solution and absorption of mineral substances insoluble in water.

The primary function of the resins[2] of Coniferæ and analogous juices of other plants is to render service in cases of injury, and, by covering the wound with a protecting coating, to favor its healing.

During my studies on the *Yucca*[3], resins and saponin were separated from each part of the plant. Experiments were made to determine the emulsive power of saponin on resins. It was found that aqueous solutions of saponin were able to emulsify many classes of resins, and in my paper it was pointed out that saponin may serve mechanical purposes in the plant as well as those of nutrition.

The succession of plants from the lower to the higher forms

[1] Cambridge Edition, p. 157.

[2] H. De Vries; *Chem. Centr. Bl.* **3**, *xiii*, 565. (*Jour. Chem. Soc.*, 1883, p. 365.)

[3] *Trans. Am. Phil. Soc.*, Dec., 1885.

will be reviewed superficially, and chemical compounds noted where they appear.

When the germinating spores of the Fungi, *myxomycetes*, rupture their walls and become masses of naked protoplasm, they are known as plasmodia. The plasmodium *Æthalium septicum* occurs in moist places, on heaps of tan or decaying barks. It is a soft, gelatinous mass of yellowish color, sometimes measuring several inches in length.

The plasmodium[1] has been chemically analyzed, though not in a state of absolute purity. The table of Reinke and Rodewold gives an idea of its proximate constitution.

Many of the constituents given are always present in the living cells of higher plants. It cannot be too emphatically stated that where "biotic" force is manifested, these colloidal or albuminous compounds are found.

The simplest form of plant life is an undifferentiated individual, all of its functions being performed indifferently by all parts of its protoplasm.

The chemical basis of plasmodium is almost entirely composed of complex albuminous substances, and correlated with this structureless body are other compounds derived from them. Aside from the chemical substances which are always present in living matter, and are essential properties of protoplasm, we find no other compounds. In the higher organisms, where these functions are not performed indifferently, specialization of tissues is accompanied by many other kinds of bodies.

The Algæ are a stage higher in the evolutionary scale than the undifferentiated non-cellular plasmodium. The simple *Alga protococcus*[2] may be regarded as a simple cell. All higher plants are masses of cells, varying in form, function and chemical composition.

A typical living cell may be described as composed of a cell wall and contents. The cell wall is a firm, elastic membrane closed on all sides, and consists mainly of cellulose, water, and inorganic constituents. The contents consists of a semi-fluid colloidal substance, lying in contact with the inner surface of the membrane,

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[1] *Studien über das Protoplasm*, 1881.

[2] Vines, p. 1. Rostafinski; *Mem. de la Soc. des Sc. Nat. de Cherbourg*, 1875. Strasburger: *Zeitschr.*, XII, 1878.

and like it closed on all sides. This always is composed of albuminous substances. In the higher plants, at least, a nucleus occurs embedded in it, a watery liquid holding salts and saccharine substances in solution, fills the space called the vacuole enclosed by the protoplasm.

These simple plants may be seen as actively moving cells, or as non-motile cells. The former consist of a minute mass of protoplasm, granular and mostly colored green, but clear and colorless at the more pointed end, and where it is prolonged into two delicate filaments called cilia. After moving actively for a time they come to rest, acquire a spherical form and invest themselves with a firm membrane of cellulose. This firm, outer membrane of the *Protococcus* accompanies a higher differentiation of tissue and localization of function than is found in the plasmodium.

Hæatococcus and plasmodium come under the classes Algæ and Fungi of the Thallothyta group. The division[1] of this group into two classes is based upon the presence of chlorophyl in Algæ and its absence in Fungi. Gelatinous starch is found in the Algæ; the Fungi contain a starchy substance called glycogen, which also occurs in the liver and muscles of animals. Structureless bodies, as *æthalium*, contain no true sugar. Stratified starch[2] first appears in the Phanerogams. Alkaloids have been found in Fungi, and owe their presence doubtless to the richness of these plants in nitrogenous bodies.

In addition to the green coloring matter in Algæ are found other coloring matters.[3] The nature[4] of these coloring matters is usually the same through whole families, which also resemble each other in their modes of reproduction.

In form, the Algæ differ greatly from filaments or masses of cells; they live in the water and cover damp surfaces of rocks and wood. In these they are remarkable for their ramifications and colors and grow to a gigantic size.

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[1] *Botany;* Prantl and Vines. London, 1886, p. 110.

[2] For the literature of Starch, see p. 115, *Die Pflanzenstoffe*, von Hilger and Husemann.

[3] *Kützing; Arch. Pharm. xli*, 38. Kraus and Millardet; *Bul. Soc. Sciences. Nat.*, Strasbourg, 1868, 22. Sorby; *Jour. Lin. Soc. xv*, 34. J. Reinke; *Jahrb. Wissenscht. Botan.*, *x*, B. 399. Phipson; *Phar. Jour. Trans. clxii*, 479.

[4] Prantl and Vines, p. 111.

The physiological functions of Algæ and Fungi depend upon their chemical differences.

These facts have been offered, simple as they are, as striking examples of chemical and structural opposition.

The Fungi include very simple organisms, as well as others of tolerably high development, of most varied form from the simple bacillus and yeast to the truffle, lichens, and mushrooms.

The cell membrane of this class contains no pure cellulose, but a modification called fungus cellulose. The membrane also contains an amyloid substance, amylomycin.[1] Many of the chemical constituents found in the entire class are given in *Die Pflanzenstoffe*.[2]

Under the *Schizomycetes* to which the *Micrococcus* and *Bacterium*[3] belong are found minute organisms differing much in form and in the coloring[4] matters they produce, as that causing the red color of mouldy bread.

The class of lichens[5] contains a number of different coloring substances, whose chemical composition has been examined. These substances are found separately in individuals differing in form. In the *Polyporus*[6] an acid has been found peculiar to it, as in many plants special compounds are found. In the Agariceæ the different kinds of vellum distinguish between species, and the color of the conidia is also of differential importance. In all cases of distinct characteristic habits of reproduction and form, one or more different chemical compounds is found.

In the next group of the Musiceæ, or mosses, is an absence of some chemical compounds that were characteristic of the classes

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[1] L. Crie; *Compt. Rend.*, *lxxxviii*, 759 and 985. J. de Seynes, 820, 1043.

[2] Page 279.

[3] M. Nencki and F. Schaffer. N. Sieher; *Jour. Pract. Chem.*, 23, 412.

[4] E. Klein; *Quar. Jour. Micros. Science*, 1875, 381. O. Helm; *Arch. Pharm.*, 1875, 19–24. G. Gugini: *Gaz. Chem.*, 7, 4. W. Thörner; *Bul. Ber.*, *xi*, 533.

[5] *Handbook of Dyeing.* By W. Crookes, London, 1874, p. 367. Schunck; *Ann. Chem. Pharm.*, *41*, 157; *54*, 261; *61*, 72; *61*, 64; *61*, 78. Rochelder and Heldt, *Ibid*, *48*, 2; *48*, 9. Stenhouse, *Ibid*, *68*, 57; *68*, 72; *68*, 97, 104; *125*, 353. See also researches of Strecker; O. Hesse; Reymann; Liebermann; Lamparter; Knop and Schnedermann.

[6] Stahlschmidt.

just described. Many of the albuminous substances are present. Starch[1] is found often in large quantities, and also oily fats, which are contained in the oil bodies of the liverworts; wax,[2] organic acids, including aconitic acid, and tannin which is found for the first time at this evolutionary stage of the plant kingdom.

The vascular Cryptogams are especially characterized by their mineral composition.[3] The ash is extraordinarily rich in silicic acid and alumina.

Equisetum,[4]	silicic acid	60 per cent.
Aspidium,	" "	13 " "
Asplenium,	" "	35 " "
Osmunda,	" "	53 " "
Lycopodium,[5]	" "	14 " "
"	alumina	26 to 27 " "
"	manganese	2 to 2·5 " "

These various plants contain acids and compounds peculiar to themselves.

As we ascend in the plant scale, we reach the Phanerogams. These plants are characterized by the production of true seeds, and many chemical compounds not found in lower plants.

It will be convenient in speaking of these higher groups, to follow M. Heckel's[6] scheme of plant evolution. All these plants are grouped under three main divisions: apetalous, monocotyledonous, and dicotyledonous, and these main divisions are further subdivided.

It will be observed that these three main parallel columns are divided into three general horizontal planes.

On plane 1, are all plants of simplicity of floral elements, or parts; for example, the black walnut with the simple flower contained in a catkin.

On plane 2, plants which have a multiplicity of floral elements, as the many petals and stamens of the rose; and finally, the higher

[1] E. Treffner; *Inaugur. Diss. Dorpat*, 1880.

[2] W. Pfeffer; *Flora*, 1874.

[3] *Die Pflanzenstoffe*, p. 323. W. Lange; *Bul. Ber.*, *xi*, 822.

[4] *Ann. Chim. Phys.*, *41*, 62, 208; *Ann. Chim. Pharm.*, *77*, 295.

[5] Flückiger; *Pharmakognosie*. Kamp; *Ann. Chim. Pharm.*, *100*, 300.

[6] *Revue Scientifique*, 13 Mars, 1886.

plants, the Orchids among the monocotyledons and the Compositæ among the dicotyledonous plants, come under the third division of condensation of floral elements.

It will be impossible to take up in order for chemical consideration all these groups, and I shall restrict myself to pointing out the occurrence of certain constituents.

I desire now to call attention to chemical groups under the apetalous plants having simplicity of floral elements.

Cassuarina equisetifolia[1] possibly contains tannin, since it is used for curing hides. The bark contains a dye. It is said to resemble *Equisetum*[2] in appearance, and in this latter plant, a yellow dye is found.

The *Myrica*[3] contains ethereal oil, wax, resin, balsam, in all parts of the plant. The root contains in addition fats, tannin and starch, also myricinic acid.

In the willow and poplar,[4] a crystalline, bitter substance, salicin or populin, is found. This may be considered as the first appearance of a real glucoside, if tannin be excluded from the list.

The oak, walnut, beech, alder, and birch contain tannin in large quantities; in the case of the oak ten to twelve per cent. Oak galls yield as much as seventy per cent.[5]

The numerous genera of pine and fir trees are remarkable for ethereal oil, resin, and camphor.

The plane[6] trees contain caoutchouc and gum; peppers,[7] ethereal oils, alkaloids, piperin, white resin and malic acid. *Datisca cannabina*[8] contains a coloring matter and another substance peculiar to itself, datiscin, a kind of starch, or allied to the glucosides.

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[1] *Dictionary of Economic Plants.* By J. Smith. London, 1882, p. 294.

[2] *Ibid*, p. 160. *Pharmakognosie des Pflanzenreichs*, Wittstein, p. 736. *Ann. Chem. Pharm.*, *77*, 295.

[3] Rabenhorst; *Repert. Pharm.*, *lx*, 214. Moore; *Chem. Centralbl.*, 1862, 779, Dana.

[4] Johansen; *Arch. Pharm.*, **3**, *ix*, 210. *Ibid*, **3**, *ix*, 103. Bente; *Berl. Ber.*, *viii*, 476. Braconnot; *Ann. Chim. Phys.*, **2**, *44*, 296.

[5] Wittstein; *Pharm. des Pflanzenreichs*, p. 249.

[6] John; *Ibid*, p. 651.

[7] Dulong, Oersted, Lucas, Poutet; *Ibid*, p. 640.

[8] Braconnot; *Ann. Chim. Phys.*, **2**, *3*, 277. Stenhouse; *Ann. Chim. Pharm.*, *198*, 166.

Upon the same evolutionary plane among the monocotyledons, the dates and palms[1] contain in large quantities special starches, and this is in harmony with the principles of the theory. Alkaloids and glucosides have not yet been discovered in them.

Other monocotyledonous groups with simplicity of floral elements, such as the Typhaceæ, contain large quantities of starch; in the case of *Typha latifolia*,[2] 12·5 per cent., and 1·5 per cent. gum. In the pollen of this same plant, 2·08 per cent. starch has been found.

Under the dicotyledonous groups, there are no plants with simplicity of floral elements.

Returning, now, to apetalous plants of multiplicity and simplification of floral elements, we find that the Urticaceæ[3] contain free formic acid; the hemp[4] contains alkaloids; the hop,[5] ethereal oil and resin; the rhubarb,[6] crysophonic acid, and the begonias,[7] chicarin and lapacho dyes. The highest apetalous plants contain camphors and oils. The highest of the monocotyledons contain a mucilage and oils, and the highest dicotyledons contain oils and special acids.

The trees yielding common camphor and Borneol are from genera of the Lauraceæ family; also sassafras camphor is from the same family. Small quantities of Stereoptenes are widely distributed through the plant kingdom.

The Gramineæ, or grasses, are especially characterized by the large quantities of sugar and silica they contain. The ash of the rice hull, for example, contains ninety-eight per cent. silica.

The Ranunculaceæ contain many plants which yield alkaloids, as *Hydrastia canadensis*, or Indian hemp, *Helleborus*, *Delphinum*, *Aconitum*, and the alkaloid berberine has been obtained from genera of this family.

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[1] *Pflanzenstoffe*, p. 412.

[2] Lecocq; Braconnot; *Pharmacog. Pflan.*, p. 693.

[3] Gorup-Besanez.

[4] Siebold and Brodbury; *Phar. Jour. Trans.* **3**, 590, 1881, 326.

[5] Wagner; *Jour. Prakt. Chem.*, *58*, 352. E. Peters, v. Gohren; *Jahresb. Agric.*, *viii*, 114; *ix*, 105; *v*, 58. *Am. Jour. Pharm.* **4**, 49.

[6] Dragendorff; *Pharm. Zeitschr. Russ.*, *xvii*, 65–97.

[7] Boussingault; *Ann. Chim. Phys.* **2**, *27*, 315. Erdmann; *Jour. Pract. Chem.* *71*, 198.

The alkaloid[1] furnishing families belong, with few exceptions, to the dicotyledons. The Colchiceæ, from which is obtained veratrine, form an exception among the monocotyledons. The alkaloids of the fungus have already been noted.

[2] Among the greater number of plant families, no alkaloids have been found. In the Labiatæ none has been discovered, nor in the Compositæ among the highest plants.

One alkaloid is found in many genera of the Loganiaceæ Berberine in genera of the Berberidaceæ, Ranunculaceæ, Menispermaceæ, Rutaceæ, Papaveraceæ, Anonaceæ.

Waxes are widely distributed in plants. They occur in quantities in some closely related families.

Ethereal oils occur in many families, in the bark, root, wood, leaf, flower and fruit; particularly in Myrtaceæ, Laurineæ, Cyperaceæ, Crucifereæ, Aurantiaceæ, Labiatæ and Umbelliferæ.

Resins are found in most of the higher plants. Tropical plants are richer in resins than those of cold climates.

Chemical resemblance between groups, as indicating morphological relations has been well shown. For example; the similarity[3] of the viscid juices, and a like taste and smell among Cactaceæ and Portulaceæ, indicate a closer relationship between these two orders than botanical classification would perhaps allow. This fact was corroborated by the discovery of irritable stamens in *Portulaca* and *Opuntia*, and other genera of Cactaceæ.

Darwin[4] states, that in the Compositæ the ray florets are more poisonous than the disc florets in the ratio of about 3 to 2.

Comparing the Cycadeæ and Palmæ, the former are differently placed by different botanists, but the general resemblance is remarkable, and they both yield sago.

Chemical constituents of plants are found in varying quantities during stated periods of the year. Certain compounds present at one stage of growth are absent at another. Many facts could be brought forward to show the different chemical composition of plants in different stages of growth. The *Thuja occidentalis*,[5] in

[1] *Die Pflanzenstoffe*, p. 21.

[2] *Ibid.*

[3] Meehan; *Proc. Acad. Nat. Sciences.*

[4] Different forms of flowers on plants of the same species. Introduction.

[5] Meehan; *Proc. Acad. Nat. Sciences.*

the juvenescent and adult form, offers an example where morphological and chemical differences go hand in hand. Analyses of this plant under both conditions show a striking difference.

Different parts of plants may contain distinct chemical compounds, and the comparative chemical study of plant orders comprises the analysis of all parts of plants of different species.

For example; four portions of the *Yucca angustifolia*[1] were examined chemically; the bark and wood of the root and the base and blades of the leaves. Fixed oils were separated from each part These were not identical; two were fluid at ordinary temperature, and two were solid. Their melting and solidifying points were not the same.

This difference in the physical character and chemical reaction of these fixed oils may be due to the presence of free fatty acid and glycerides in varying proportions in the four parts of the plants. It is of interest to note that, in the subterranean part of the *Yucca*, the oil extracted from the bark is solid at the ordinary temperature: from the wood it was of a less solid consistency; while the yellow base of the leaf contained an oil quite soft, and in the green leaf the oil is almost fluid.

Two new resins were extracted from the yellow and green parts of the leaf. It was proposed to name them *yuccal* and *pyrophæal*. An examination of the contents of each extract showed a different quantitative and qualitative result.

Saponin was found in all parts of the plant.

Many of the above facts have been collected from the investigations of others. I have introduced these statements, selected from a mass of material, as evidences in favor of the view stated at the beginning of this paper.[2] My own study has been directed towards the discovery of saponin in those plants where it was presumably to be found. The practical use of this theory in plant analysis will lead the chemist at once to a search for those compounds which morphology shows are probably present.

I have discovered saponin in all parts of the *Yucca angustifolia*

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[1] H. C. DeS. Abbott; *Trans. Amer. Philos. Soc.*, 1886.

[2] For further facts confirming this theory, see "Comparative chemistry of Higher and Lower Plants." By H. C. DeS. Abbott. *Amer. Naturalist*, August, 1887.

in the *Y. filimentosa* and *Y. gloriosa*, in several species of Agavæ, and in plants belonging to the Leguminosæ family.

The list[1] of plants in which saponin has been discovered is given in the note. All these plants are contained in the middle plane of Heckel's scheme. No plants containing saponin have been found among apetalous groups. No plants have been found containing saponin among the lower monocotyledons.

The plane of saponin passes from the Liliaceæ and allied groups to the rosales and higher dicotyledons.

Saponin belongs to a class of substances called glucosides. Under the action of dilute acids, it is split up into two substances, glucose and sopogenin. The chemical nature of this substance is not thoroughly understood. The commercial[2] product is probably a mixture of several substances.

This complexity of chemical composition of saponin is admirably adapted for the nutrition of the plant, and it is associated with the corresponding complexity of the morphological elements of the plant's organs According to M. Perrey[3], it seems that the power of a plant to direct the distribution of its carbon, hydrogen, and oxygen to form complex glucosides is indicative of its higher functions and developments.

The solvent action of saponin on resins has been already discussed. Saponin likewise acts as a solvent upon barium[4] sulphate and calcium[5] oxalate, and as a solvent of insoluble or slightly soluble salts would assist the plant in obtaining food, otherwise difficult of access.

Saponin is found in Endogens and Exogens. The line dividing these two groups is not always clearly defined. Statements pointing to this are found in the works of Haeckel, Bentham, and others.

Smilax belongs to a transition class, partaking somewhat of the

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[1] Different genera and species of the following: Ranunculaceæ, Berberidaceæ, Carophyllaceæ, Polygalaceæ, Bromeliaceæ, Liliaceæ, Smilaceæ, Yuccas, Amaryllideæ, Leguminosæ, Primulaceæ, Rosaceæ, Sapindaceæ, Sapotaceæ.

[2] Kobert; *Chem. Ztg.*

[3] *Compt. Rend., xciv*, p. 1124.

[4] *Bul. de la Soc. Chim.*

[5] "Yucca angus." *Trans. Am. Philos. Soc.*, Dec., 1885.

nature of Endogen and of Exogen. It is worthy of note that this intermediate group of the sarsaparillas should contain saponin.

It is a significant fact that all the groups above named containing saponin belong to Heckel's middle division.

It may be suggested that saponin is thus a constructive element in developing the plant from the multiplicity of floral elements to the cephalization of those organs.

It has been observed that the composite occurs where the materials for growth are supplied in greatest abundance, and the more simple forms arise where sources of nutrition are remote. We may gather from this fact that the simpler organs of plants low in the evolutionary scale contain simpler non-nitrogenous chemical compounds for their nutrition.

The presence of saponin seems essential to the life of the plant where it is found, and it is an indispensable principle in the progression of certain lines of plants, passing from their lower to their higher stages.

Saponin is invariably absent where the floral elements are simple ; it is invariably absent where the floral elements are condensed to their greatest extent. Its position is plainly that of a factor in the great middle realm of vegetable life, where the elements of the individual are striving to condense, and thus increase their physiological action and the economy of parts.

It may be suggested as a line of research to study what are the conditions which control the synthesis and gradual formation of saponin in plants. The simpler compounds of which this complex substance is built up, if located as compounds of lower plants, would indicate the lines of progression from the lower to the saponin groups.

In my paper[1] read in Buffalo at the last meeting of the American Association for the Advancement of Science, various suggestions were offered why chemical compounds should be used as a means of botanical classification.

The botanical classifications based upon morphology are so frequently unsatisfactory, that efforts in some directions have been made to introduce other methods.[2]

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[1] *Botanical Gazette*, October, 1886.

[2] Borodin ; *Pharm. Jour. Trans.*, *xvi*, 369. Pax. Firemy ; *Ann. Sci. Nat. xiii.*

There has been comparatively little study of the chemical principles of plants from a purely botanical view. It promises to become a new field of research.

The Leguminosæ are conspicuous as furnishing us with important dyes, *e. g.*, indigo, logwood, catechin. The former is obtained principally from different species of the genus *Indigofera*, and logwood from the *Hæmatoxylon* and *Saraca indica*.

The discovery[1] of hæmatoxylin in the *Saraca indica* illustrates very well how this plant in its chemical, as well as botanical, character is related to the *Hæmatoxylon campechianum*; also, I found a substance like catechin in the *Saraca*. This compound is found in the *Acacias*, to which class *Saraca* is related by its chemical position as well as botanically. Saponin is found in both of these plants as well as in many other plants of the Leguminosæ. The Leguminosæ come under the middle plane or multiplicity of floral elements, and the presence of saponin in these plants was to be expected.

From many of the facts above stated, it may be inferred that the chemical compounds of plants do not occur at random. Each stage of growth and development has its own particular chemistry.

It is said that many of the constituents found in plants are the result of destructive metabolism, and are of no further use in the plant's economy. This subject is by no means settled, and even should we be forced to accept that ground, it is a significant fact that certain cells, tissues or organs peculiar to a plant, secrete or excrete chemical compounds peculiar to them, which are to be found in one family, or in species, closely allied to it.

It is a fact that the chemical compounds are there, no matter why or whence they came. They will serve our purposes of study and classification.

The result of experiment shows that the presence of certain compounds is essential to the vigor and development of all plants and particular compounds to the development of certain plants. Plant chemistry and morphology are related. Future investigations will demonstrate this relation.

In general terms, we may say that amides and carbo-hydrates are utilized in the manufacture of proteids. Organic acids cause a turgescence of cells. Glucosides may be a form of reserve food material.

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[1] H. C. DeS. Abbott, *Proc. Acad. Nat. Sciences*, Nov. 30, 1886.

Resins and waxes may serve only as protection to the surfaces of plants; coloring matters, as screens to shut off or admit certain of the sun's rays; but we are still far from penetrating the mystery of life.

A simple plant does what animals more highly endowed cannot do. From simplest substances they manufacture the most complex. We owe our existence to plants, as they do theirs to the air and soil.

The elements carbon, oxygen, hydrogen, and nitrogen pass through a cycle of changes from simple inorganic substances to the complex compounds of the living cell. Upon the decomposition of these bodies the elements return to their original state. During this transition those properties of protoplasm which were mentioned at the beginning, in turn, follow their path. From germination to death this course appears like a crescent, the other half of the circle closed from view. Where chemistry begins and ends it is difficult to say.

ON ANEMOMETERS.

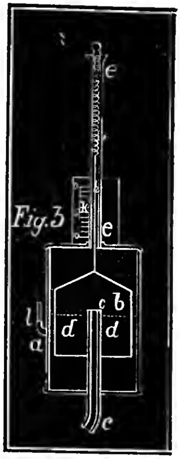
BY G. A. HAGEMANN, Candidatus Polytech.

[*Translated by G. E. Curtis, from the "Annuaire météorologique" of the Danish Meteorological Institute, Copenhagen, 1877.*]

INTRODUCTION.—As the Hagemann anemometer has attracted some attention, and is easily obtained by purchase of the maker, Nyrop, of Copenhagen, and as the original memoir describing it is rather rare, I have, in compliance with several requests, suggested to the Franklin Institute, the propriety of publishing the following translation.

It is evident that Dr. Hagemann's preliminary investigation needs further development before the value of the instrument can be considered to be finally established. For measuring severe gusts and high wind velocities, an anemometer which has a minimum inertia of its own moving parts is very desirable, and this seems to be obtainable through the application of the principle of suction. CLEVELAND ABBE.

A series of experiments upon the drafts of chimneys, for which I used a gauge constructed for the purpose, drew my attention to the fact that the draft up the chimney depends upon the force of the wind, so that each gust of air, no matter how feeble, that grazed the summit of the chimney, could be perceived in the apparatus for measuring the draft. Thus occurred the idea of utilizing the instrument as an anemometer, and I think I have established by the following experiments, its applicability to the simple and relatively very exact indication of the force of the wind, and thereby to the determination of the velocity.



The apparatus, as shown in *Fig. 3*, is substantially as follows:

In a reservoir, *a*, filled with water up to the level marked *l*, there is suspended by a wire or silk-thread a thin metallic bell, *b*; the upper end of the wire is fastened to a finely-tempered spring. A slit cut in the tube, containing the wire and spring, shows at *e* a small needle attached to the wire, whose motion is read off on a divided scale, *k*.

If now the metal-bell be given a diameter of 0·1128 m., or a section of 100 square centimetres, a suction in the tube *c*, whose top is above the water, equivalent in amount to one millimetre of water pressure, will produce a tension of ten grammes on the spring, which latter will be stretched thereby.

By choosing a spring of suitable force, the needle can easily be made to move ten millimetres for a pressure of ten grammes, and

thus the suction, as indicated on the scale, will be amplified ten times. The use of wheel-work admits of converting the original rectilinear motion into circular motion, and of amplifying still more the indications. If instead of suction, we wish to measure pressure, it is only necessary to load the bell, *b*, so as to make it sink, and the intensity of the wind-pressure can then be measured by the height to which the bell is raised, when the air in the tube *c* is compressed.

As a receiver of the wind, I have used a Pitot tube, *i. e.*, a tube with its aperture facing the wind, of 6 mm. interior diameter and with nicely sharpened edge, and also the tube designed by Captain Magius, of Copenhagen. The latter is simply a vertical tube, acting like a chimney, across whose top, also with sharpened edge, the wind blows at right angles to the axis of the tube.

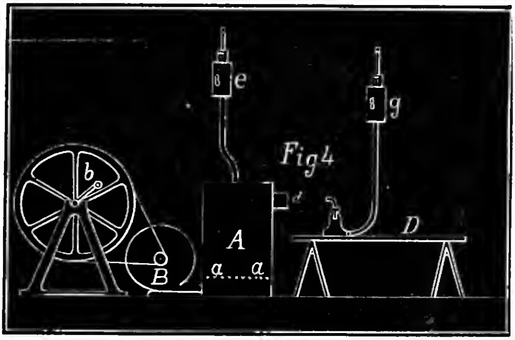


Fig4

In order to produce the currents of air requisite in these experiments, an apparatus was used represented in *Fig. 4*. The reservoir *A*, furnished with a grating *a . . . . . . a* is constantly fed with compressed air by a fan worked by hand. The pressure is such as the experiment requires. A manometer, *e*, is so placed that the person working the fan can constantly follow with his eye the variations of pressure in the reservoir *A*. This latter contains an orifice *d*, of 9·5 cm. interior diameter that can be fitted with tubes of different diameters. On the Table *D* are placed the various receivers, connected by rubber tubes with the apparatus, *g*, for measuring pressure or suction.

After having compared the gauges, and having found them to agree within a limit of error not exceeding 0·1 mm., I fixed a Pitot tube, 6 mm. aperture, facing a jet of air which emerged from *A*

through a round orifice 1·7 cm. in diameter. It then became manifest that the Pitot tube, as indicated by the manometers, *always* gave a pressure identical with the statical pressure in the reservoir A, when the distance of the tube to the orifice of escape did not exceed 5 cm. Experiments were made with pressures varying from 0 to 100 mm. of water and gave results agreeing perfectly.

From Weisbach's *Mechanik*, the velocity, v, of a current of air passing through an orifice in a thin plate is,

$$v = 396 \sqrt{(1 + \delta t) \frac{h'}{b + h'}} \text{ metres,}$$

where h' is the pressure in the reservoir expressed in millimetres of mercury.

Since the Pitot tube indicates a pressure precisely equal to that which forces the air to flow with the velocity v, the above formula ought also to indicate the velocity of a current of air producing an effect h' in the gauge attached to the Pitot tube.

As, moreover, the temperature, t, is the same in the whole mass of air in motion, and as h' is always a very small quantity compared with $b = 760$ mm., we can simplify the formula and substitute the approximation:

$$v = 396 \sqrt{\frac{h}{760 \times 13{\cdot}598}} = 3{\cdot}9\sqrt{h} \text{ metres,}$$

where h is the pressure in millimetres of water, indicated by the manometer.

The diminution of pressure indicated by the Pitot tube, when its axis is oblique to the direction of the wind has been made the subject of observation and my results are contained in the following table.

TABLE I.

Distance from d, 30 cm. Diameter of d, 6 cm. Pressure in A, 10 mm.

Angle.	*Pressure in Pitot Tube.*
	mm.
0	6·1
10	6·1
20	6·0
30	5·75

The variations are so small that, up to an angle of 30° between

the direction of the wind and the axis of the tube, they do not exceed a few per cent.

The use of the Pitot tube, then, makes possible the construction of an anemometer that will operate with simplicity and precision; farther on, after having mentioned some difficulties that anemometers of this kind present, I will indicate more in detail the best method of construction.

Before beginning a review of my experiments made with the Magius tube, it will not be out of the way to examine more closely the current of air which emerges from an orifice in a thin plate, and to review the following experiments.

TABLE II.

Pressure in A, 8 mm.

Distance from d.	*Pressure in Pitot Tube.*	*Distance from d.*	*Pressure in Pitot Tube.*
cm.	mm.	cm.	mm.
0.25	8·	9·	5·5
1·	8·	10·	4·45
2·	8·	15·	2·1
4·	8·	21·5	1·4
5·	8·	26·	1·1
6·	7·5	30·	0·75
7·	6·9	41·6	0·6
8·	6·35		

Whence it results in detail that for a distance from the orifice less than 5·5 cm., the latter having a diameter of 1·7 cm., a Pitot of 7 mm. diameter indicates the same pressure at the centre of the current of air as in the interior of the reservoir *A*. But if this distance be exceeded, the pressure begins to diminish rapidly, because the jet of air drags along the surrounding air in its course, and so loses velocity. It is only at a distance of 40 cm. that the mass of air seems to flow with a pretty uniform motion. Further experiments will throw more light on the distribution of pressure in the air-current.

The vertical tubes proposed by Captain Magius have not yet, so far as I know, been subjected to precise investigation, and consequently it was necessary, as a preliminary step, to know just how far the size and shape of the orifice exerts an influence, and also at what limits the indications remain proportional to the wind

velocity. Such was the object of the experiments presented below:

TABLE III.

Distance from *d*, 6 cm. Diameter of *d*, 2 cm.

Pressure in A,	*Suction in Tubes of different Sizes and Shapes.*					
	7 mm.	6 mm.	4 mm.	2 mm.	1 mm.	4 mm.
						
mm	mm.	mm.	mm.	mm.	mm.	mm.
1	0·4	0·4				0·4
2	0·7	0·7	0·7	0·6	0·5	0·7
3	1·1	1·1				
4	1·5	1.5	1·55	1·2	1·0	1·5
5	1·9	1·9				
6	2·3	2·4	2·4	1·8	1·5	2·4
7	2·9	2·9				
8	3·4	3·4	3·4	2·9	2·0	3·25

These experiments show that all the Magius tubes having an aperture of 7 to 4 mm., give results which agree very closely, and impress a certain stamp of continuity on the curve that is obtained by making the pressure in *A* an abscissa and the suction an ordinate. For very small capillary tubes, on the contrary, such is no longer the case. The energy of suction diminishes with the diameter, and there is no continuity in the indications. A cause, however, which contributes to this irregularity, is found in the fact that the experiments were not continued a sufficiently long time, as is apparent from the following series of experiments in which the first and third tubes had their edges sharpened, but the middle tube not.

TABLE IV.

Distance from *d*, 6·5 cm. Diameter of *d*, 8 cm. Pressure in *A*, 10 mm.

Duration of Experiment.	*Suction in Magius Tubes.*		
minute.	5 mm. diam.	1·25 mm. diam.	1·25 mm. diam.
0·5	5·25	2·0	2·0
1·0	6·1	3·5	3·4
1·5	6·3	4·25	4·25
2·0	6·4	4·7	5·0
3·0	6·4	4·9	5·5
4·0		5·0	5·7
5·0		5·1	5·75
6·0		5·1	5·75

A Magius tube, even of 5 mm. diameter, takes an appreciable

time to rarefy all the enclosed air, and consequently it is not surprising that the capillary tubes require still more time. The reason that these latter tubes do not come to indicate a suction as great as tubes of larger calibre ought assuredly to be found in the influence of the capillary walls on the air. The following result then obtains: Capillary tubes are poorer receivers of the wind, and ought to be employed only so far as they can be shown to be more suitable than larger tubes in keeping free from obstruction in all kinds of weather.

Magius tubes of large diameter give, suctions proportional to the velocity of the wind, at least up to 8 mm. of pressure in *A*.

Beyond that limit I have been obliged to content myself with using simple curved glass tubes as gauges, instead of the form of manometer previously described.

TABLE V.

Distance from *d*, 6 cm. Diameter of *d*, 2 cm.

Pressure in A.	*Suction in Magius Tubes.*			
	7 mm. diam.	7 mm. diam.	5 mm. diam.	mean.
7·		3·0		3·0
8·	3·4			3·4
11·5		5·0		5·0
16·		7.0		7·0
20·	7·5	9·0		8·25
30·	11·0		12·0	11·5
40·	14·0		17·0	15·5

In spite of the comparatively large uncertainty in these measurements, nevertheless, I venture to assume, that further experiments will show that the ratio is maintained at least up to 100 mm. of pressure in *A*, and perhaps even beyond that limit. The action of the Magius tube is to be explained as being the result of cohesion between the molecules of the air current, which touches the open end of the tube and those of the air contained in the tube.

That is the reason we ought to find in these tubes the total effect which corresponds to different velocities, so long as cohesion does not vanish, which never occurs until a perfect vacuum is reached. It is presumable, however, that the higher velocities demand an appreciable time for the suction to attain its full value, but I have not yet measured this time.

If then, relying on the experiments presented in Tables III and V, one should conclude that the velocity of the wind preserves a

definite ratio to the suction measured in the Magius tube, and consequently that it could be expressed by

$$v = 3{\cdot}9 \sqrt{a\,h} \text{ metres,}$$

he would be in error, because what we have really measured in these experiments is not the velocity of the current of air or the suction that the current is able to produce, but, on the contrary, *the difference between the suction and the pressure shown by the air-jet.* In order that a current of air shall open a way for itself in a mass of quiet air, it must do mechanical work, and consequently at every point must undergo a compression or exercise a pressure which is proportional to the work to be performed. It is this pressure which affects the indications of a Magius tube; if we contract the orifice of escapement, there is added to these causes a contraction of the air-jet. By changing the diameter of the orifice, and consequently the pressure of the jet, we obtain—

TABLE VI.

Distance from *d*, 6·5 cm. Diameter of *d*, 9·5 cm.

Pressure in A.	*Suction in Magius Tubes.*	*Pressure in A.*	*Suction in Magius Tubes.*
1	1·0	6	4·75
2	1·8	8	6·25
4	3·3	10	7·75

So long as the velocity of the air current is small, and consequently the pressure of the jet light, the Magius tubes indicate a suction almost equal to the pressure in the reservoir *A*, but in proportion as the velocity and, with it, the pressure of the jet increases, this very pressure reacts on the suction, but to a less degree than that observed in experiments III and V, because now the orifice is very much larger. We can now draw the conclusion that the Magius tube cannot afford a very exact measure of wind gusts, because such gusts behave in the same way with respect to surrounding air, as the air-jet in these experiments. These gusts must contain compressed air, which naturally renders them incapable of producing in the Magius tube a suction as great as it ought to be by reason of their velocity. The following experiment will set forth, in a most striking manner, the distribution of pressure in the jet:

TABLE VII.

Diameter of *d*, 1·7 cm. Pressures in *A*, 8 mm.

Distance from d.	*Pitot Tube*, 6 mm.	*Magius Tube*, 6 mm.	*Pressure in Jet.*
0·25	8·0	1·8	6·2
4·0	8·0	2·9	5·1
6·0	7·5	3·4	4·1
10·	4·5	2·9	1·6
17·	2·0	1·6	0·4
21·5	1·5	1·2	0.3
26·	1·1	1·0	0·1
30·	0·75	0·75	0·0
41·6	0·6	0·6	0·0

I have shown above that experiments III, V and VI cannot lead to the expression of the velocity of the wind measured in the Magius tube. The conditions of experiment are such as do not admit of obtaining empirically the true expression, because we cannot produce an artificial current of air in which there is no additional pressure. However, we can arrive at it approximately. As the tables show, the pressure of the jet must be less at the opening of large orifices than at that of small ones, because the Magius tubes indicate with the latter a larger suction If, then, we look for some law, according to which the size of the orifice exerts an influence on the suction, or on the diminution of pressure in the jet, we shall reach the desired result. The following experiment throws light on this point :

TABLE VIII.

Distance from *d*, 30 cm. Pressure in *A*, 10 mm.

Diameter of d.	*Pitot Tube*, 6 mm.	*Magius Tube*, 6 mm.
2·		1·0
4·2		3·25
6·		4·6
8·	6·6	5·75
9·5	6·75	6·35

If the squares of the diameters, *d*, be laid off on the axis of abscissas and the suctions be used as ordinates, we obtain a continuous curve. (*Fig. 5.*)

Just as this curve manifestly tends, with the increase of abscissas, to become parallel to the axis of abscissas, so we see it, in addition, approaching a portion of the curve generated in the same manner with the Pitot tube, and we cannot doubt that, by sufficiently increasing the abscissas, even up to the point where the air,

rushing through the orifice, constitutes a mass animated with a uniform motion, *we shall obtain, both from the Pitot tube and the Magius tube, indications of pressure in the first and of suction in the second, corresponding exactly to the pressure under which the air-current is expelled from the reservoir A.*

As experiments with natural and uniform air-currents have confirmed the correctness of this conclusion, the velocity of the wind, as measured with a Magius tube, will be expressed by the same formula as in the case of the Pitot tube, namely:

$$V = 3{\cdot}9 \sqrt{h},$$

where h is the suction expressed in millimetres of water, indicated by the gauge.

When the air-current does not move normally to the orifice of the Magius tube, we shall no longer have, as one could suppose

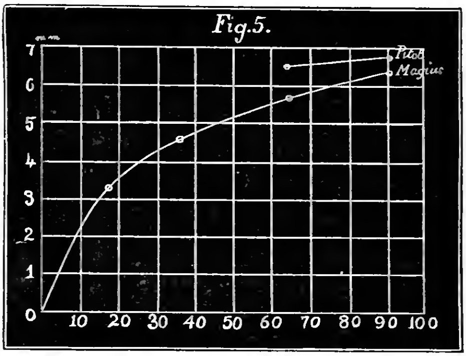


Fig. 5.

in advance, the indication given by the whole velocity of the wind. In the following table are given the measurements obtained for different angular displacements.

TABLE X.

Distance from d, 6·5 cm. Diameter of d, 2 cm. Pressure in A, 8 mm.

Deviation of Tube from the Vertical.		*Magius Tube.*	
→			
<	>	mm.	mm.
6°		3·4	
	6°	3·4	
6°			3·1
10°			2·8
	10°		3·3
⊥			3·35

Distance from d, 30·0 cm. Diameter of d, 9·5 cm. Pressure in A, 10 mm.

Deviation of the Tube from the Vertical.		*Magius Tube.*
→		
<		mm.
⊥		6·5
6°		6·5
	6°	6·5
12°		6·25
	12°	6·25
15°		5·70
	15°	5·40

Any small deviations from the vertical made by the tube are, from the above, insignificant. If, however, the angle amounts to 15°, the resulting effect assumes sensible proportions. We see also that it is preferable to give the tube a very sharp bevelled edge, and to admit only a very small thickness at the lips of the orifice.

If now we take a glance at all the preceding measurements, and the conclusions deduced therefrom, we arrive at the following results:

(1.) The Pitot tube can form the basis of a good, precise anemometer, which is only a very little affected by small deviations from parallelism with the direction of the wind.

(2.) The Magius tube, except with capillary diameters, constitutes a good receiver of the wind for constant velocities, but is not able to register accurately gusts of wind, because the latter are composed of compressed air. The Magius tube will not admit any great deviation from the vertical.

I have found these results fully justified by experiment with natural winds. So long as the wind blows uniformly, the Pitot and Magius tubes are on exactly the same footing, but as soon as there comes a gust, their agreement disappears, the two gauges then begin at once to indicate the change, but whilst the manometer connected with the Pitot tube rapidly attains its maximum, a noticeable time elapses before the apparatus, which measures the suction in the Magius tube, can keep pace with it, and it is only when the gust reaches the condition of a uniform wind of higher velocity that the Magius tube comes finally to give the same change as the Pitot tube. Many gusts are of so short duration that one can scarcely observe them by means of their feeble instantaneous

suction in the Magius tube, which, besides, is not at all a true measure of their velocity.

While for the reasons presented, the Magius tube gives as a general result too weak indications, yet there are two other factors which ought to be mentioned as tending partly to counter-balance this error; namely, the force of expansion of the air in the tube itself, when the latter is in any place having a temperature higher than that of the surrounding air, or when it is heated by the sun; second, the oscillations of the tube. The first source of error is common to both Pitot and Magius tubes, and it is easy enough to make a correction for it; but as to the latter, which is complicated in its action, it is hard to give any general rules. So far as I have been able to observe, it is not the oscillation that takes place in the direction of the wind which increases the suction, because its effect should cancel itself (that is why the oscillation has no perceptible effect in the case of the Pitot tube), but it is any oscillation that makes an angle with the direction of the wind. Suppose the wind sweeps over the upper open end of the tube, and suppose that the tube oscillates in the air, the effect should be the same, because it is the relative velocity which is the cause of the suction.

In order to estimate the amount of the error, I have used a Magius tube, 10 cm. long, set in a rubber collar, so as to be oscillated by a strong current of air and I found—

TABLE XI.

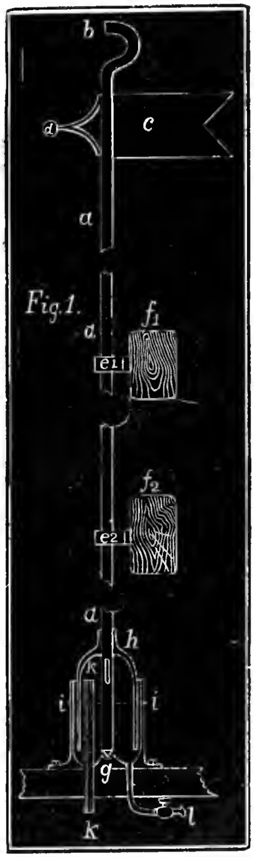
	Pitot Tube.		*Magius Tube.*	
Distance from d.	*Oscillating.*	*Fixed.*	*Oscillating.*	*Fixed.*
0·15	50	50	54	47
0·40	50	51	57	51
0·70	35	35	45	37
1·00	24	24	24	24
0·40	90		93	
0·40	85		95	
0·40	80		90	

Although the oscillation of the tube was scarcely visible, and although the tube was not a long one, yet the error is so considerable in the Magius tube, that the suction could evidently not be used for giving an accurate determination of the wind velocity.

However, I do not regard it as impossible, whenever the opportunity presents itself, to determine at least approximately the amount of error, by calculating the duration of the oscillations

from the length of the tube exposed to the weather; the experiment would not be long in making known the amount of the effect produced by different velocities.

The Magius tube presents, on the whole, by reason of its very simple form, so many advantages that, at least in places where up to the present time the observer has been obliged to be satisfied



with estimating the force of the wind, he will be able to derive benefit by using this tube as an anemometer. If a precise anemometer is desired then we must use the Pitot tube, and I will venture to recommend the following construction, *Fig. 1*.

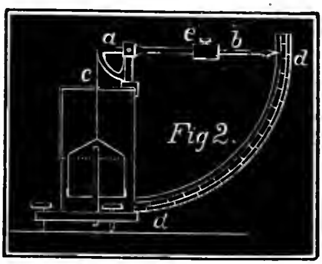
The iron tube *a* carries at the top the Pitot tube *b* as well as the wind-vane *c* and its counterpoise *d*. At e_1 and e_2 the tube is

fastened by collars fixed to the beams f_1, f_2. The bottom of the tube ends in a steel point which moves in the socket g; a bell h is fixed on the tube and is partly submerged in oil which surrounds the lower end of the tube, and is contained in a reservoir i. Within the bell, but above the oil, the tube is perforated with a series of holes through which the pressure of the wind received in the Pitot tube is transmitted to the bell where the tube K conducts it to the manometer.

In order to draw off the water which comes through the tube a in storms, the tube and cock, l, is used.

The cost of such an apparatus is small. The instrument can be set up so that there shall be no danger of its being overturned by high winds, and for precision, it leaves nothing to be desired.

For a manometer I should recommend using an arrangement a little different from that used in these experiments, to wit, the kind



of letter-balance shown in *Fig. 2*. The bell rests in use as before but in place of a spring, the steel-guard a–b is used, of which the short arm a carries an arc of a pulley on which rests the silk thread c, whilst the long arm moves over the divisions of a scale graduated on another arc d. By giving this arc three or more different graduations which, according to circumstances, correspond to different positions of the weight e, the same apparatus can be used to measure pressure or suction expressed as height of water from 0 to 120 mm., and that, too, with a precision proportional to the smallness of the quantities.

GLASS-MAKING.

By C. HANFORD HENDERSON, Professor of Chemistry and Physics, Philadelphia Manual Training School.

[*A Lecture delivered before the* FRANKLIN INSTITUTE, *Monday, January 10, 1887.*]

PROF. HENDERSON was introduced by Dr. Persifor Frazer, Professor of Chemistry in the INSTITUTE, and spoke as follows:

LADIES AND GENTLEMEN:—It is related that when the Queen of Sheba went to visit Solomon, that astute monarch so arranged his audience throne that the Queen and her suite in approaching would be obliged to pass over a floor of glass, under which was flowing water and fishes swimming. For the legend has it that the wisest of men was decidedly curious. Having heard that his queenly guest labored under the disadvantage of a deformed foot, his ingenuity suggested the device of the flowing water, thinking that the lady's anxiety for her draperies would discover to the King of the Israelites and his court whether rumor had rightly reported her. But I am much disposed to ascribe this performance to the imagination of one later than Solomon. Not only would so inhospitable an act have been notably at variance with the royal genius, but at that time it would scarcely have been possible. Not even the 120 talents of gold and the very great store of spices and precious stones which the admirer of wisdom brought with her as a present to Jerusalem could have purchased a plate of glass sufficiently large and sufficiently clear to have made such a deception possible. In the production of curious works of art in glass, and the fabrication of rare bits of colored ware, the ancients showed themselves scarcely inferior to modern glass-makers, but the magnificent sheet of glass through which we of a morning study the signs of the weather, or admire the tempting display in the shop windows, is a luxury peculiar to our own times. If our age had not already been devoted successively to the genius of iron, of steel, and latterly of electricity, I think we might designate it, not without reason, the "age of glass," so manifold have been the applications of this material.

That we must, however, ascribe to glass a great antiquity is beyond question. Some ingenious investigators have carried its origin back to the time of Tubal Cain, the patron of the metallurgist's art, but their following has been very small. The most commonly received story is that told by Pliny and Tacitus, which ascribes the origin of glass to an accidental discovery made by a company of Phœnician merchants. These, it seems, had landed on the sandy coast of Palestine, and in heating their cooking vessels over the fire, made use of soda cakes taken from their cargo, no stones being available for the purpose. The sand and alkali being brought together in the fire, united to form a transparent fluid, and thus, say these historians, the first glass became known. But the formation of even a soda glass with no greater heat than that given by an exposed wood fire is open to chemical question. We have, however, much stronger testimony than this careless hearsay, which shows the fabrication of glass to have been an accomplished fact in the earliest historical times. To Egypt, the home of most of the arts and sciences, we must look for the earliest examples of the blower's skill. On several of the ancient tombs, scenes are depicted which represent unquestionably different stages in the manufacture of glass. But what is even more conclusive, interesting examples of this early art have also been discovered. A glass bead, found at Thebes, and described by Sir Gardner Wilkinson, in his *Manners and Customs of the Ancient Egyptians*, formed at one time a part of a royal necklace. It has engraved upon it the name of Queen Hatason, the wife of Thotmes III, who reigned 1500 B.C. The industry, thus early established, seems to have attained a national importance, for when the country was subdued by Cæsar Augustus, he decreed that a part of the annual tax should be paid in glass. The Romans were not slow in introducing so advantageous an art into Italy, and many interesting examples of their workmanship are now to be found in the museums of Europe. Venice, in particular, became celebrated for the delicate ingenuity of its glass-workers. The ateliers of Murano preserved for many years the best secrets of the blower's art. The gradual spread of the industry throughout the world at large, and especially its development in the countries of Europe, form a story of much interest, and one that I regret time will not permit me to repeat. But even confining our inquiry in time to the present, and

in locality to America, there will be much that must of necessity be omitted. It is to be remarked, however, of these early manufacturers, that for many years glass seems to have been regarded as a material better suited to the requirements of the fine arts than to the demands of every-day life. Hence, it was to be found in the possession of few, save the very wealthy. With this usage in view, the early glass-makers gave more attention to brilliancy of lustre and depth of coloring than to the more useful qualities of transparency and size. In this, it cannot be denied that they met with marked success. Some of the color effects in glass of the Twelfth Century cannot be imitated at the present day, and it is even whispered that not a few of the famous gems in the historic crowns of Europe owe their origin to the crucibles of these early chemists rather than to the laboratory of Nature.

The term glass calls to our mind an amorphous solid, at once hard and brittle, and varying in its translucency to the most beautiful transparency on the one hand, and on the other to absolute opacity. It is a mixture of different silicates, compounds of silicic acid, with the bases soda, potash, lime, magnesia, alumina, iron and lead. Every true glass contains at least two bases united with the silica and generally, by virtue of the impurities associated with the crude materials, traces of several more. We have, therefore, grown into the habit of designating the different kinds of glass by the names of the two principal bases. Thus, we speak of window glass as a lime-soda glass; of flint glass, used for optical instruments, as a lead-potassium glass; of the well-known Bohemian product, as a potash-lime glass, and so on through the list. It is a valuable property of these mixed silicates that they have a fusibility much below the mean of the constituent salts. Thus, while the silicates of alumina, lime and magnesia are almost infusible alone, they become quite manageable when associated with the silicates of soda and potash.

The operation of glass-making is one which involves not only considerable skill in the chemic art, but also not a little familiarity with the principles of physics. I scarcely know which to admire the more, the nicety with which the glass-maker regulates the proportions of his charge, so as to produce this beautifully clean substance, or the dexterity with which he handles the finished product, and adapts it to our uses. The several steps in the pro-

cess of glass-making depend from the very beginning—the choice of the raw materials—upon the purposes to which the glass is to be put. While there is great similarity in the operations of melting, blowing, moulding and annealing, the differences in the several manipulations are sufficiently marked to make it desirable that each special branch of glass manufacture shall be described separately. The processes of fabrication can better be classified by referring them to the character of the product, than to the constitution of the glass. Following this principle, we will find that all the more common varieties of manufactured glass will be included in the following table:

MANUFACTURED GLASS.

- Flat Ware.
  - Blown.
    - Sheet Glass,
    - Crown Glass,
  - Cast.
    - Plate Glass.
    - Mosaic Glass.
    - Optical Glass.
  - Pressed.
    - Decorative Pieces.
- Hollow Ware.
  - Blown Glass.
  - Pressed Glass.

I shall consider flat ware first, as being far the more important of the two main classes. I think you will agree with me that, however convenient and helpful glass may be when applied to the construction of domestic or scientific utensils, its use for these purposes sinks into utter insignificance when compared to its much larger value in filling the windows of our houses, in lengthening our days to the dimensions assigned by Nature, and in permitting us to enjoy the sunshine of out-door life at all seasons of the year without being exposed to the inclemencies of the weather; or its more subtle use in optical instruments, in giving sight to the almost blind, in endowing the infinitesimal world, through the aid of the microscope, with sensible proportions; or by means of the telescope, bringing the infinite regions of space within the scope of human observation. When these uses of the material come up before the mind, glass becomes not only an object of interest from the skill required in its fabrication, but also an object of reverent wonder from the larger universe it has made possible to us.

Among the many industries that have been benefited by the utilization of natural gas, there is probably none in which the results have been so marked as in the manufacture of window glass. For a number of years past, American sheet glass has been

undoubtedly inferior to the product of European factories, and has consequently occupied but a secondary position in the estimation of our builders and architects. The foreign makers, and more particularly those of France and Belgium, have hitherto shown a superior skill in the management of their materials. They seem to have held the secret of obviating the bad effects of impurities in their fuel. This result has been made possible from their greater experience in the industry, and from a better construction of furnaces. In the more perfect plants, crude fuel has been abandoned, and manufactured gas used in its place, thus anticipating the advantages of natural gas, with the important exception, however, of its cheapness and almost total freedom from sulphur. The more favorable conditions prevalent abroad made imported glass synonymous with best quality. That the circumstances of the industry have now so far changed, that American glass-makers can successfully compete with foreign producers of the best reputation, and can even claim certain points of superiority for the home product, is a subject for hearty congratulation. The well-known ingenuity of American inventors has, in a measure, effected this improvement, but perhaps the substitution of gaseous fuel has been the most potent cause of our recent successes. The metamorphosis of the crude material into a clear and brilliant pane of glass involves about the same operations in all our modern factories, but in each establishment the details of manufacture are slightly varied. I shall therefore call your attention to-night to the fabrication of sheet glass as carried out at Pittsburgh, for in that city one can see the best American practice.

The manufacture of window glass depends for its success upon the closest attention to details, and its history is therefore one of delicate manipulations. It is a very easy matter simply to make glass. Sand, alkaline bases and heat are the only elements needed to accomplish the transformation. The iron-master frees his ores from their associated gangue by making it into a fusible silicate of lime and alumina, an opaque glass. The assayer separates his metal from its impurities by adding suitable fluxes until all earthy matters are gathered into a fusible slag, and float above the metallic button as a molten glass. And even Nature, when her local caldrons bubble over in the volcanos, shows that she too is a giant glass-maker. But to make good glass; glass that shall be

clear, transparent, colorless; that shall simulate the purest water of the mountain stream—this requires skill and patience, and not in one part of the process alone, but in all, from the mixing of the crude materials to the annealing of the finished product, the glass-maker must be alert and intelligent.

Window glass, as we have seen, is a lime-soda glass, or a mixture of the silicates of lime and soda. It approaches quite nearly to the composition represented by the chemical formula $CaO, Na_2O, 6\,SiO_2$. To supply the ingredients required by this formula, the raw material, or "batch" employed, consists of about thirty parts of lime, forty of alkali, and a small but varying amount of pulverized charcoal to each 100 parts of sand, the commercial representative of silica. These are thoroughly ground and mixed together before being introduced into the furnace. Some manufacturers make their alkali all sulphate of soda, while others employ a mixture of sulphate and carbonate in the proportions shown by their experience to give the best results. Not only does the composition of the alkali vary greatly, but the relative amounts of the three components of the batch are different in every establishment, and even in the same establishment vary in accordance with the quality of the crude materials.

Where gas is used, the construction of the melting furnace is very simple. A plain rectangular floor or hearth gives support to eight or ten glass pots, standing two abreast; a series of round openings on each side of the furnace permits free access to each pot; the gas is admitted at each end and is mixed with air which has previously been heated by passing through chambers in the fire-brick arch. An intense heat is thus obtainable, and one that has the advantage of being under the most complete control. A well is built under the furnace in order to collect the molten glass should a pot break, and so avoid loss of material or stoppage of the work. An arch is provided at each end of the furnace to permit the admission or removal of the pots. When the furnace is in blast the opening is closed by fire-bricks and luted with clay.

The manufacture of crucible pots is the most tedious and exacting process connected with glass-making. It requires constant care, for if the treatment be in any way imperfect, the entire subsequent work of the crucible will be unsatisfactory. At Pittsburgh, the pots are generally made up of a mixture of two parts raw fire-clay, two parts burned fire-clay, and one part ground pot shells. The

well-ground mixture is placed in lead-lined bins or troughs, and sufficient water added to make the mass plastic. It is turned once a day for a period of about four weeks. The workman kneads the mass with his bare feet in order to make it tough and free from air. In this country the pots are generally formed by hand, the temperature and humidity of the work-room being kept as nearly constant as possible. The bottom of the pot is first formed, and then the sides built up gradually from day to day, the entire process occupying about six weeks. The uncompleted walls are always left covered with damp cloths in order to prevent premature hardening. The pots are ordinarily made 33½ inches deep, and 42½ inches across the top. The thickness varies from 3½ inches at the base to 3 inches on top, while the bottom is 4 inches. Their capacity is from fourteen to sixteen hundred pounds of molten glass. When the pots have been completed, they are are permitted to stand in the work-room for several months in order to dry very gradually. They are then placed in small heating furnaces, where the temperature is slowly raised to that of the melting furnace. The transfer from one to the other is made as quickly as possible. The interior of the crucible is then glazed with molten glass, and is ready to receive the raw materials.

One-third of the charge is first placed in the pots, and allowed to melt before the addition of the rest of the batch. If the furnace is in good condition, the melting proceeds from below upwards, the cone of raw material gradually sinking into the bath of molten glass. If this does not occur, if the fusion begins on top, it is a very plain indication that the heat has not been properly regulated, and that a long period will be required to accomplish the complete melting of the charge. At the end of about four hours another third is added, and after a similar interval of time, the remainder of the batch is finally introduced. About two pounds of arsenious acid are put in with the last charge, in order to bleach the glass by converting the iron present into a higher oxide. At some establishments, the peroxide of manganese is used to accomplish the same purpose, but it has the disadvantages of giving the glass a pinkish color if used even in slight excess. It is also believed to make the transparency of the glass less durable.

Some years ago, an excess of manganese was employed intentionally, in the manufacture of window glass as it was thought that

a pretty face looked prettier when seen through rose-colored glass. Some of you doubtless remember having seen this decidedly pink glass in not a few of the older houses of the city. It may indeed be seen at the present time even, for its use was revived by the severe hail storm of seventeen or eighteen years ago, when the neglected manganese glass was again brought into requisition by the emergency.

After the contents of the pot have become quite liquid, a capping of broken glass is added to fill them up completely. The entire melting of such a charge occupies about sixteen hours. During the latter portion of this period, the heat is somewhat reduced to make the glass less liquid, and prepare it for gathering. But first, the surface of the molten "metal" must be freed from all impurities by skimming. A fire-clay ring, which was introduced into the pot when it was first put in the furnace, floats upon the bath, and the gatherer, by removing all the scum from the interior of this ring, always has a clear surface from which to draw. The glass is gathered on the end of a wrought-iron blow-pipe, about five feet long, the end of which is decidedly flared. The first dip brings out but a small lump of glass, which is gotten into symmetrical oval shape by a careful turning of the pipe. Three times the process is repeated, until the gatherer has a mass of from fifteen to twenty pounds of glass on the end of his pipe. When window glass of double thickness is to be made, the metal must be gathered as many as four or five times. The resultant ball in this case weighs from thirty to forty pounds. It is at the final dip that the gatherer's greatest skill is called into requisition. To get the mass of red-hot plastic glass into symmetrical shape, and satisfy himself that it is thoroughly homogeneous throughout, he rests his pipe on a convenient fulcrum, and by a rapid revolution while the end carrying the glass is still in the furnace, causes the last glass added to completely overlap the former ball. The entire mass is brought almost to the liquid condition, and by a skilful manipulation of the blow-pipe, the fold of glass is turned into a spiral and worked to the end of the mass. The red-hot ball of glass is now taken to a wooden mould, and by a few dexterous turns is formed into a pear-shaped ball. The mould is kept from burning by being constantly moistened with water, which, in contact with the heated glass, assumes a spheroidal condition, and looks like so many

globules of mercury. When this has been accomplished, the gatherer's duty is at an end, and he hands pipe and glass over to the blower.

In France and Belgium, the same furnace is generally used for both melting and blowing, but in England and this country it is found not only more convenient, but even more economical to use separate furnaces. The blowing furnace adapted for gaseous fuel is similar in many respects to that used for melting. It is constructed with a series of side openings, somewhat larger in diameter than those of the former, and simply provides an intensely hot chamber for controlling the temperature of the glass while being blown. The gas, however, instead of being introduced at each end, is burned directly under the openings, or blow-holes. The requisite amount of air is mixed with the gas by means of fire-clay chimneys surrounding the burners in a manner precisely similar to the chimney in the Bunsen burner. In order to prevent the flame from impinging directly upon the glass being manipulated, fire-clay slabs or bricks are placed a short distance above each burner, and thus divide the flame into harmless jets.

In the most completely equipped works, the division of labor is carried into thorough practice. Each man knows how to do one particular thing, and does it. The blower, for instance, into whose hands the red-hot ball of glass has just been consigned, knows nothing of crude materials, melting processes or molten baths. Nor, on the other hand is he supposed to have more than a vague conception of what is meant by a pane of glass. His crude material is the pear-shaped mass on the end of the blow-pipe; his finished product, a large cylinder of glass. The skill with which he affects his part of the many transformations required in the genesis of a window pane is, however, the most attractive in a process nowhere devoid of interest. His first act is to grasp the pipe, and with the ball of glass still resting in the mould, blow through the mouth-piece until a large bubble of air is formed in the mass. Then, with alternate blowing and manipulating, he increases the bubble, until the mass assumes a shape not unlike that of the large carboys used in the transportation of acids. On each side of the furnace, and directly in front of the openings or blow holes, there is a wide platform, the long openings in which, running at right angles to the furnace, permit the blower to swing

his pipe and ball of glass in a pit beneath. Blowing, swinging and heating, he extends the bubble, until in place of the ungainly carboy, with its disproportionately thick bottom, he has a beautifully symmetrical figure, the shape of an enormous test tube. From time to time, however, during these operations, it happens that the glass flows a little too freely, and that there is danger of the sides of the cylinder becoming too thin. To avoid this result, the blower throws his cylinder into the air whenever he finds that the glass is too liquid, and so permits it to settle back upon itself. The tube being by this time about five feet long, and the blow-pipe as many more, one can readily fancy that this apparently playful toss requires both skill and muscle. In the case of the larger cylinders, such as will furnish a pane 66 x 54 inches, and which must be made of double thickness, the labor is so great that few men are found who are capable of its performance.

When the tube has been formed to the satisfaction of the blower, he allows it to become comparatively cool. He then thrusts the end into the furnace, blows into his pipe, and quickly covers the mouth-piece with his hand. A slight report is soon heard. The end has become softened with the heat, and the confined air, expanding with the increasing temperature, has blown a hole in the glass. Resting his pipe on a suitable support, and still keeping the glass in the furnace, the blower gradually turns it around. Under the influence of this centrifugal force, the hole grows larger and larger, until he no longer has a test tube at all, but in its place an open cylinder. This is quickly withdrawn from the furnace, and permitted to depend into the pit below, until the plastic edge passes to a cherry heat, and the cylinder can be taken away without danger of getting out of shape.

The blower's part is now completed, and after a moment's rest, he has another pipe in his hand, and is repeating his heavy labor.

The neck of the cylinder and its attached blow-pipe are separated from the cylinder proper by wrapping around the end of it a thread of red-hot glass, and after its removal, applying a piece of cold iron to any point heated by contact with the thread of glass. A red-hot iron is also passed along the interior surface from end to end, making a longitudinal crack; or the same result may be effected by means of a diamond attached to a long handle. We have now a perfect cylinder, open at both ends, and having a crack

its entire length. Another step in its transformation into a window pane has been accomplished.

The cylinder is now taken to a separate building to what is known as the laying-in furnace. The hearth is made circular, and is divided into a number of sectors, separated from each other by fire-clay bridges. As the hearth revolves, the different sectors move through as many separate compartments of the furnace, the temperature of which may be varied at pleasure. The first compartment, which is only moderately warm, is known as the laying-in oven, and permits the cylinder to become gradually heated. A partial revolution of the hearth then carries it to the next compartment, the laying-out oven, where the temperature is sufficiently elevated to make the glass plastic. A large flat stone, manufactured out of fire-clay, prepared with the greatest care, occupies the floor of each hearth sector, and is adapted to receive the cylinder. In the laying-out oven, the crack is brought uppermost, and under the influence of the heat, the cylinder gradually unfolds until it lies open on the stone like a sheet of rumpled paper. In the next compartment, the flattening oven, a workman irons out the plastic sheet with a moistened block of wood on the end of a long rod until it is perfectly smooth and flat. The smoothed sheet, by another revolution of the hearth, is taken to the compartment known as the dumb oven, where it slowly cools. A final revolution of the hearth brings it to the entrance of the annealing leer, next door to the laying-in oven, thus making the circuit complete. The process, you see, is quite continuous, and by a few simple operations transforms the cylinder into a flat sheet of glass. But still it is not ready for use. Were the glass taken from the dumb oven and permitted to cool in the air, it would be so brittle that it would be almost without value. It must therefore go through the process of annealing, or gradual cooling, before it can become serviceable.

The most improved annealing oven is that known as the "rod leer," which has come into general use in Pittsburgh, and other localities where the best practice is followed. When the glass reaches this stage of its journey, it is picked up with a large two-pronged fork, and is placed upon a series of rods projecting from the mouth of the leer. These are found an immense improvement over the cars formerly used for the purpose. They handle each

sheet separately, and are so arranged that when it is desired to make room for a fresh sheet, a part of the rods may be raised and carry the contents of the entire leer towards the cooler end, where all the sheets are eventually discharged. The glass remains in the leer from thirty to forty minutes, in place of several hours or days, as in the old-fashioned annealing ovens. When the glass is discharged, it is nearly or quite cold, and may be at once cut into proper sizes and stored in suitable frames.

This, with the exception of the important commercial transaction of converting the glass into money, completes the process in window-glass manufacture. In all departments of the work, the advantages derived from the use of gaseous fuel are becoming each day more evident. If you will examine, even casually, the differences between gas-made glass and the older article made with coal, you cannot help being struck with the manifest superiority of the new product. The surface of the glass, just as it comes from the furnace, is remarkably brilliant, and quite as beautifully clear as if it had been washed with hot water by some careful housekeeper, and dried with linen. A better and more thorough fusion is obtained from the more intense heat of the gaseous fuel, and, what is even more important, the contamination of the "metal" by particles of coal and cinder is entirely avoided. In the latter part of the process, in flattening out the glass cylinders, the advantages of gas are particularly manifest. When coal was used, the sheets of glass came from the laying-out oven covered with smoke, and infinitely worse than that, a white deposit of sulphur. It must be remembered that these impurities were gathered while the glass was in a semi-plastic condition, and that in consequence, no subsequent washing or acid bath could entirely restore its brilliancy. The contrast between the two fuels, gaseous and solid, is perhaps still better shown by a glance at the history of those establishments which are not so fortunate as to possess it. Quite a number of large glass works throughout the West have admitted that the competition with the factories supplied with natural gas is too unequal, and have either suspended operations or have transferred themselves to the shadow of the nearest gas derrick. Several such migrations have been reported during the past year, and where this has not been possible, manufactured gas has in a number of cases taken the place of the crude fuel.

The manufacture of crown glass, though commercially much less important than that of sheet glass, possesses considerable historical interest, and within the past year or two has been brought into some prominence again from the use of the material in decorative windows. It possesses a brilliancy far superior to that of its younger rival, but the small size and unequal thickness of the panes obtainable, do not permit it to successfully compete with the generous dimensions and constant uniformity of the sheet glass. The glass itself is alike in both, the differences between the two being due entirely to the subsequent manipulations, after the melting process has been completed. As before, the glass by several successive gatherings is collected on the end of the blow-pipe, and by rolling on a table of metal or stone, known as the marver, is gotten into the shape of a cone, the apex of which forms the so-called "bullion point." The workman now blows into his pipe, expanding the glass into a small globe. This is subsequently enlarged, care being taken to keep the bullion point in the line of the blow-pipe. The globe is then flattened to something of the shape of an enormous decanter, the bottom being very flat, and having the bullion point in its centre. The pipe and its burden are now permitted to rest horizontally upon two iron supports. In the meantime, another workman has gathered a small lump of glass upon the end of his iron rod or "pouty," and by pressing it against an iron point, has impressed upon it the shape of a small cup. This is fitted over the bullion point of the glass, and soon becomes firmly attached to it. The blow-pipe is separated from the glass by means of cold iron and a sharp blow. The open neck thus exposed is known in the glass-worker's parlance as the "nose," and gives its name to the furnace where it is subsequently re-heated. During this operation, the pouty is constantly and rapidly revolved. Under the combined action of heat and centrifugal force the nose gradually expands, the opening growing larger and larger until the piece has the shape of a typical crown. But this appearance remains only an instant, and in its place is seen a brilliant circular plate of glass, whose shape is only maintained by continuing the rotation of the pouty until the plate, or table as it is now called, can be laid upon a flat support. The pouty is then detached from the bull's-eye by means of shears. As soon as they are sufficiently cool to be rigid, the tables are stacked in annealing

ovens where they remain from one to two days. Their diameters vary from a few inches to six feet, but the latter dimension is extreme. After annealing, they are divided by a diamond into two unequal parts, the larger of which contains the bull's-eye. It can readily be imagined that a semi-circle of glass, which has even the extreme radius of three feet, cannot be cut into square panes very advantageously, and this consideration, together with the small sizes necessary in crown glass, have more than counter-balanced its admirable brilliancy.

At the present time, crown glass, in the circular form, just as it comes from the annealing oven, is being used in decorative windows with very excellent effect. The glass is frequently tinted, amber being a special favorite, or else it is white, with the bull's-eye colored. A very effective window of this sort may be seen in the hallway of the Tiffany Glass Works, in New York. It consists simply of a succession of crown glass tables, perhaps eight to ten inches in diameter, having opalescent and tinted bull's-eyes. The use of the bull's-eye alone is also becoming quite popular in mosaic window glass.

Sheet and crown glass are the chief representatives of the blown ware in the flat. I have described their manufacture in some detail, from the feeling that the former, at least, is the most important of all the products of the glass-maker's art. In the next division of our subject, we shall be brought to a consideration of a class of products, those obtained from casting, which are far more beautiful and wonderful than the former, which, since they affect the welfare of a smaller proportion of the civilized world, must be ranked economically of less importance.

Following the order given in our table, we shall next take up the manufacture of plate glass, and for this purpose I shall again ask your presence in Pittsburgh. This time, however, our visit will be to Creighton, some twenty miles north of the city, and near to the well-known natural gas district of Tarentum. There are in this country four large establishments where plate glass is manufactured. The Creighton plant has the reputation, however, of enjoying the most favorable economic conditions, and it would certainly be difficult to find in this or any other country one more completely equipped. The glass itself has the same constitution as the sheet and crown glass. It is simply a double silicate of lime

and soda. The melting is carried out in large open pots, the furnaces differing in their construction from those already described, only in their greater size and the substitution of doors made of fire-clay tiles set in cast-iron frames for the usual gathering holes. When the fusion has been completed, the door opposite the pot is opened, and a two-pronged fork, mounted on wheels, is inserted into the furnace. The distance between the prongs is sufficient to permit them to pass into depressions made in each side of the melting pot, and thus secure it in a firm grasp. By this method, the pot of molten glass is removed from the furnace, and is carried on a low truck to the casting table. At Creighton, the casting house, containing furnaces, tables, and annealing ovens, is 650 x 160 feet, about four times as large as the famous *halle* of St. Gobain, in France, and nearly double the size of the British Works at Ravenhead. There are two casting tables at Creighton, 7 inches thick, 19 feet long, and 14 feet wide. Each is provided with an iron roller, 30 inches in diameter, and 15 feet long. Strips of iron on each side of the table afford a bearing for the rollers, and determine the thickness of the plate of glass. The tables are mounted on wheels, and run on a track which reaches every furnace and annealing oven. The table having been brought as near as possible to the melting furnace, the pot of molten glass is lifted by means of a crane, and its contents quickly poured on the table. the heavy iron roller is then passed from end to end, spreading the glass into a layer of uniform thickness.

As rapidly as possible, the door of the annealing oven is opened and the plate of glass introduced. The door is then closed, and the glass left to anneal. All of these operations are performed in little more time than it takes to describe them, as it is desirable to get the glass into the annealing oven as hot as can be. A large number of ovens are required for annealing purposes, as the glass must remain several days to cool. When the glass is taken out, its surface is found to be decidedly rough and uneven. A small quantity is used in this condition for sky-lights and other purposes where strength is required without transparency. It is known in the market as rough plate. The greater part of the glass is ground, smoothed and polished before it leaves the works. The grinding is accomplished by means of rotary grinding machines, the abrading material being common river sand dredged from the

Alleghany. Three million bushels are required annually for this purpose. The plates are firmly fixed on large rotary tables or platforms by means of plaster of Paris. Rotating discs are so arranged that they cover the entire surface of the glass at each rotation of the platform. Small jets of water keep the grinding sand always wet. These operations remove the rough exterior. The smoothing is accomplished by emery, finer and finer grades being used as the process proceeds. The final polishing is done by means of rouge (carefully calcined sulphate of iron). The monthly product of the Creighton plant is about 100,000 square feet of glass. The fuel throughout the entire works is natural gas, which here displaces about 3,000 bushels of coal daily. It is used in melting furnaces and annealing ovens, as well as in supplying the steam for engines of about 1,500 aggregate horse-power. These figures will give you some idea of the magnitude of the operations connected with a large factory and will perhaps dispel the notion, if such exist, that we are largely dependent upon France for our supply of plate glass. The output of this factory, though so large, finds ready market and is never greater, I understand, than the demand; for the American plate glass can compete both in quality and price with that of European make. At Creighton, a part of the output is utilized in the manufacture of mirrors, and improved bevelling machinery has been introduced in order to give the glass the desired finish.

The subject of colored glass windows is a very large one, and whether viewed either from the artist's or technologist's standpoint would be difficult to exhaust. In its nomenclature, we have permitted ourselves to fall into rather careless habits. The terms "painted," "stained" and "mosaic" glass are used indiscriminately to designate any glass work involving color, but a moment's consideration will show them to be far from synonymous. Some of our best effects are produced without the use of either paint or stain, and they have the advantage of a much greater durability. In painted glass the colors are obtained by enamels fused to the surface of the glass by means of heat. In stained glass, a permanent, transparent color effect is secured by the action of heat on certain metallic oxides applied to the surface as pigments. In mosaic glass, pure and simple, the design is brought out by the use of shaped fragments of colored glass bound together by strips of

doubly-grooved lead. The three products, you see, are quite distinct. It frequently happens, and in the older examples of ecclesiastical design it is nearly always the case that all are combined in one window. But at the present time there is a strong reaction against the employment of either stain or paint, since they are less durable and less brilliant than homogeneous colored glass. The tendency is very decided to rely entirely upon the mosaic treatment, and to limit the use of paint to the representation of the human body. Even here it is reduced to a minimum by employing a translucent glass and shading sparingly in monochrome. A light reddish-brown is the favorite tint. It has the disadvantage of giving a statue-like sameness to all the figures. Should the present taste continue, our picture windows promise to become an assemblage of rather monotonous blonde types.

The manufacture of mosaic glass at the present time has attracted the attention of men of such ingenuity and taste that it deserves its rank among the fine arts. It has attained a degree of artistic perfection, of which the earlier examples gave little promise. In spite of the abandonment of paint and stain, the mosaic glass has been given greater variety and greater depth of color than at any time since the Renaissance. The glass itself has been made in all the colors of the spectrum, and has undergone a thousand different transformations. By the mixing of several colors when the glass is no longer liquid, curious mottled effects have been produced, while the addition of cryolite and other indissoluble substances has given us the opalescence so much admired in the art glass work of the last few years. The shapes have been no less varied than the colors. The so-called "jewels," or pieces of richly-colored glass, cut with facets after the manner of precious stones, have added immensely to the brilliancy of modern designs. I had recently the pleasure of going through a factory for colored glass in Brooklyn, probably the largest establishment of the kind in this country, and I assure you that it was a chromatic treat to visit their storerooms, for 500 different color combinations were recognized in their stock. The mosaic ateliers of the Vatican contain, it is true, some 26,000 different tints; but these, you must remember, are simply opaque enamels, while the glass of which I speak is all easily translucent, and much of it is clearly transparent. Time will not permit me to give you anything like an exhaustive

description of this branch of glass manufacture, but the subject is far too interesting to be passed over in silence. The basis of the process is, as before, a lime-soda silicate, the coloring being due to the addition of soluble metallic oxides. Taking them up in the order of the spectrum, the violet shades are generally produced from manganese or from small quantities of cobalt; the deep blues, indigos, purple blues, and normal blues are obtained from varying proportions of cobalt; peacock blue from copper, the finest greens from chromium and copper, and the dull sea-water tint from ferrous oxide. The yellows come from a variety of sources. The sesquioxide of uranium gives a fluorescent yellow; the oxide of lead, a pale yellow; the oxide of chromium, an emerald-green; and the oxide of silver, applied as a pigment to the surface of the glass, a permanent yellow stain. The higher oxide of iron gives an orange color, but as it has a strong tendency to become reduced, it is necessary during the manipulation of the glass to keep some oxidizing agent present, such as manganic oxide. In the reds, a number of excellent shades are readily obtainable. Manganese furnishes a variety of pinkish-reds and pinks; copper in its lower oxide, the fine blood-red of Bohemian glass, and gold, the most brilliant of all reds, the well-known ruby color. In addition to these, a number of other substances are used to produce either colors or unique effects. A little carbonaceous matter yields an amber tint of very agreeable hue, while the opalescence now so much in vogue results from the presence of oxides of tin, arsenic or lime, or from native minerals, such as fluorite, or the cryolite imported in such large quantities at the present time from Greenland. If simply colored, transparent sheet glass is to be made, the molten metal may be gathered and blown into cylinders in precisely the same way as in the manufacture of window glass, but in mosaic glass it is now much preferred that the glass employed should not be transparent, or but imperfectly so, since the color effects are much richer from uneven surfaces. The most of the glass is therefore cast, the process being a repetition in miniature of the casting of rough plate. The pots containing the molten colored glass remain, however, always in the furnace, and the metal is dipped out in small iron ladles. It is poured at once on a little casting table, and is smoothed out by means of an iron roller. The sheets being so small, are readily handled and permit the use

of the convenient rod leer. In case more than one color is to appear in the same sheet, the effect is obtained by mixing the several masses of plastic glass on the casting table by means of a copper implement not unlike a plasterer's trowel. In this way three or even four colors are mixed together in the same piece of glass, and though the results are always more or less experimental, artists have learned to adapt them not only to their geometrical designs, but also to their picture windows as well. The workmen have attained no little skill in the art of mixing. The blue and white translucent glass in particular is made to represent sky effects almost as naturally as if the colors had been laid on by an artist's brush. From the factory the glass is taken to the studio. A number of preliminary steps must be taken before the actual work of putting the glass together can begin. The artist first makes a sketch of his design, and then, if satisfactory, enlarges it to the natural size. This working drawing is then colored and divided up by broad black lines representing the strips of lead necessary to hold the pieces of colored glass together. The cutting of the glass is a severe tax upon the judgment, and has to be carried out under the immediate supervision of the artist. In geometrical designs, the requirements of color harmony alone need attention; but in picture windows, in addition to this, a very appreciative eye is needed to seize upon just the right combinations to bring out the draperies, background and sky, for no paint or stain is used in the entire picture except the monochromatic shading representing the head and other exposed portions of the figure. There are in this country a number of establishments where work of such a character is done. The Tiffany Glass Company of New York have been particularly successful in adapting the mosaic treatment to picture windows. They have recently reproduced Gustave Doré's famous painting, "Christ Leaving the Prætorium," for a church memorial window, the entire piece being executed in pure mosaic, with the exception of the faces and hands. The dimensions of this truly magnificent work of art are 20 x 30 feet. It is the most ambitious window ever attempted in America, and indeed the largest opalescent piece in the world.

The glass employed in optical instruments must needs be as dense as possible, since its refractive power increases with its specific gravity. We employ for this purpose, therefore, a mixture

of the silicates of lead and potash. But as these compounds differ greatly in their respective densities, much care must be taken to prevent their separation, and the consequent streaky structure which would result. The sand, red oxide of lead and potash, having been mixed in the proper proportions; that is, so as to produce a glass having approximately a composition represented by the formula $PbO, K_2O, 6SiO_2$, are introduced in small quantities at a time into a melting pot provided with a dome-shaped cover. This excludes smoke and other impurities, and at the same time prevents the furnace gases from reducing the lead to the metallic state. During the fusion, the mass is frequently stirred by means of a fire-clay cylinder, attached at right angles to a long iron handle. When the fusion is judged to be complete, the furnace is reduced to a lower temperature, and the melting pots permitted to remain at rest for perhaps a couple of hours, in order that all the bubbles throughout the mass of glass may come to the surface. A constant stirring is then maintained for another two hours. In the meanwhile, the temperature falls so low that the stirring towards the end of the period becomes quite difficult. When the operation ends, the clay cylinder is withdrawn, all the openings to the furnace are closed up, and crucible and contents are allowed to gradually cool. This requires about a week. The crucible is then taken out and carefully broken, so that it may be separated from the mass of flint glass. Parallel faces on the sides of the mass are ground and polished in order that the internal defects may be located, and the glass cut up to the best advantage. Those who have been interested in watching the equipment of the Lick observatory, on Mount Hamilton, in California, will perhaps remember the repeated trials that were necessary before the glass for the great telescope could be successfully cast and placed in the hands of Alvan Clarke for grinding. The subsequent processes of adapting the material obtained at the cost of so much labor and expense to optical uses, though of much interest, scarcely come within the limits of to-night's inquiry.

Of the flat ware, then, a word only remains to be spoken concerning the pressed decorative pieces now used with such excellent effect in domestic architecture, and in the ventilators of the newer designs of cars. The process of manufacture is very simple. The molten colored glass is taken from the crucible in a small ladle,

and by virtue of the rapid cooling induced by contact with the cold iron, is in a condition of plasticity by the time it reaches the press. Here it is quickly pressed between the two pieces of the mould, the excess of glass being squeezed out between them in so thin a sheet that it can readily be detached from the finished product. Quite a variety of shapes and designs are now manufactured. Rectangular pieces, stamped with simple flower or geometrical designs, are becoming quite popular for small windows and transoms. Circles, squares, and other pieces are also being made in quantities for introduction into mosaic glass work, and form an agreeable feature in the design.

In the manufacture of hollow ware in glass, we have two distinct processes producing characteristic products, the blown and the pressed glass. The first of these includes all vessels which owe their form to the blower's breath. In considering the manufacture of window glass we have seen the facility with which a mass of glass when in a plastic state may be made to expand into a hollow globe or cylinder at the will of the operator. This same agency, the blower's breath, when a little more daintily applied, furnishes our tables and laboratories with the manifold forms of glass ware which add so much to our daily convenience. In its chemical composition, the glass used for this purpose varies considerably. The celebrated Bohemian glass, which cannot be surpassed in brilliancy by crystal itself, is a silicate of potash and lime with small quantities of iron and alumina. Much of the commoner table ware is similar in its composition to window glass, but possesses little brilliancy and has frequently a greenish cast due to the presence of the lower oxide of iron. The so-called crystal, which in England is sometimes denominated flint glass, owes its weight and refractive power to the presence of silicate of lead. Like the product employed for optical purposes, it is in the main a double silicate of potash and lead, but contains less lead than the latter glass, and has consequently a lower specific gravity. It is the material employed for the fabrication of cut glass and the finer grades of table service.

In the manipulation of the plastic metal, two methods offer themselves to the choice of the glass-worker. In the one, he forms his articles entirely in the air by the dexterous use of a few simple tools, and in the other he depends upon a cast-iron mould to give

the desired shape to the exterior, while the interior is formed by the pressure of his breath. The use of moulds, though common in the manufacture of the cheaper grades of glass ware, and of much importance in bottle-making, is prohibited in the case of the finer goods since, it robs the glass of much of its brilliancy. There is a peculiar polish, which comes from working the glass in the air, not unlike that characteristic of crown glass. It is more than sufficient to compensate for the greater time required by the process. The blow-pipe used in making these smaller articles is a light wrought-iron tube, from four to five feet long and having a diameter hardly greater than one-fourth of an inch. The mass of molten glass gathered on the end of the blow-pipe is compressed and worked into symmetrical shape, by being rolled on a marver. A little air introduced into the interior of the mass transforms it into a bulb, which is then lengthened by swinging. So far, the process is the same whatever may be the ultimate form impressed upon the glass. The subsequent treatment of the bulb is determined by the shape of the article which it is desired to produce. If, for instance, a wineglass or goblet is to be made, the bulb is extended to the proper size to form the bowl, and the stem formed either by drawing out a part of the substance of the bulb itself, or by attaching a small mass of glass to the bottom of the bowl, and while still red hot, drawing it out into the desired shape. The glass worker distinguishes the first as the "straw stem," and the second as the "stuck shank." The partly formed goblet is now ready for its foot. This is either blown or cast, the choice being quite independent of the nature of the stem. The blown foot is formed on a separate blow-pipe, and when attached to the stem, is simply a bulb of glass somewhat smaller than the bowl. The bulb is then opened, and by a rapid twirling of the glass expands into a circular plate forming the foot of the wineglass or goblet. The addition of a cast foot is brought about in a somewhat different manner. A small mass of molten glass is dropped on the end of the stem, and is flattened into the requisite shape by being pressed between slabs of wood or prepared carbon. The original blow-pipe is now separated from the upper half of the bowl, and the rough edge of glass trimmed off by means of shears. In case the article to be manufactured is a pitcher, or other vessel with a handle, the hollow body is first formed, and the handle generally

added in a separate piece. This is attached at one end to the glass, and is drawn out to the desired thinness. The requisite length is then cut off and the free end made fast to another part of the glass vessel. There is in this department an immense variety of shapes and sizes manufactured, each style calling into play some particular adroitness in the management of the molten glass. All of the tools employed are extremely simple, the results depending almost entirely upon the manual dexterity of the workman.

The pressed glass in hollow ware is a variety attaining increasing importance. It is not so brilliant as the blown glass, but at the present time is made in very attractive shapes, and has the merit of low cost. The process of manufacture is similar to the pressed window pieces. The red hot plastic glass is pressed between a fixed mould and a corresponding plunger actuated by hand power. In the flat pieces, such as dishes and plates, the designs used in cut-glass have been reproduced with fair success. They can readily be detected, however, from the genuine article by their inferior brilliancy, and also from the indistinct, rounded appearance, which in glass seems inseparable from angles produced by fusion. In the case of decanters, cruets, and the like, made to imitate cut-glass by being blown in moulds, the deception is now frequently heightened by the use of real cut-glass stoppers. The practice of cutting and grinding the facets in pressed glass has prevailed to some extent, but the surfaces so treated lack the brilliancy of the uncut glass, and so gain little by the operation. The genuine cut-crystals, whose rainbow beauties have made it admired above all other products in glass, is made either from blown ware or from pressed, with perfectly plain surfaces. In this way, the cutting is made to penetrate beneath the chill produced by the mould, and to develop the full chromatic possibilities of the glass.

The manufacture of bottles is a distinct and very important division of the making of hollow ware. It is nowhere in America carried on so extensively and so successfully as in the neighborhood of Philadelphia. Much of the sand of Southern New Jersey is sufficiently pure to make an excellent bottle glass. Its adaptability for this purpose seems to have been appreciated by the early settlers, for the oldest glass works in this country are those established, in 1775, at Glassboro. These works, the property of Messrs. Whitney Brothers, are also at the present day the most

extensive, employing, as they do, some 600 persons in the conduct of their operations. It is a significant fact, showing the force of modern progress, that, after existing for more than a century, the capacity of the plant has been increased more than fifty per cent. during the past three years. As this gratifying result is largely, if not entirely, due to the introduction of improved furnaces, invented by the chemist of the works, Mr. Andrew Farrari. I shall call your attention briefly to their construction. They are, in a word, tank furnaces, heated by gas. Neither the employment of a tank in place of separate crucibles nor the substitution of a gaseous for a solid fuel is in itself new; but the details of the Ferrari furnace are quite novel.

In Europe the regenerative system of Siemens has been employed with marked success in the manufacture of glass, but unfortunately the Siemens furnaces are expensive in their construction, and require some degree of skill to ensure their best working. The Ferrari furnace is an inexpensive affair, and possesses several features of decided advantage. The gas generator is the usual inclined or "step" grate employed by Siemens, but is placed directly alongside of the furnace, thus obviating the transportation of the gas and the necessity of reheating it before being burned. The carbon of the coal is burned on the grate to carbon dioxide, and rising through the mass of incandescent fuel above it, is reduced to the monoxide, and with the volatile hydrocarbons given off passes at once to the melting chamber above the tank. The air necessary for combustion is heated by passing through chambers in the lower portion of the furnace. It is mixed with the combustible gases just before they reach the fire-clay bridge separating the generator from the melting tank. The operation of the furnace is continuous. The crude materials of the batch are introduced at intervals of three hours, about two and one-half tons making up the charge. As the material melts it sinks to the bottom of the tank and flows through small openings leading to the gathering chamber beyond. The glass resulting from the fusion of the sand and alkaline bases has a specific gravity greater than the crude material from which it is formed, and consequently seeks the lowest level. In this way, the tank, though filled with material in all stages of transformation, has always at the bottom a bath of thoroughly fused glass. The communication between tank and

gathering chamber is so arranged that the fluid glass alone can pass from one to the other. There are three Ferrari furnaces in operation at Glassboro the largest of which has a capacity of fifty tons. I am told, on very reliable authority, that not only is the quality of the glass much improved by the employment of these furnaces, but that in addition the experience of three years has shown their maintenance and operation to be notably less expensive than the old style pot furnace. During July and August, the furnaces are out of blast, as the heat is too great to permit the men to work. For ten months, however, the operations are continuous, the necessary repairs being so light, that they do not interfere with the work. In its composition, three grades of bottle glass are recognized. The ordinary green glass is obtained from a change of 100 parts of sand, eighteen parts of sodium carbonate, twenty-two parts of sodium sulphate, and twenty-four parts of lime. As no bleaching agents are employed, the iron present in the sand gives the glass its characteristic light green color. The second grade, the amber glass, has about the same composition, but is colored by the addition of about three-eighths of one per cent. of carbon. The finest of the bottle glass—the so-called flint glass—is obtained from a charge of 100 parts of sand, thirty-five parts of refined soda, and twenty parts of limestone. Manganese dioxide, arsenious acid and nitrate of soda are used as bleaching agents. The bottles are formed entirely in moulds, the blower's breath giving shape to the interior. Where the bottles are very small, one man is able to blow as many as 400 dozen in a day, but this means very dexterous work.

The glass markets of to-day are particularly rich in unique products. The fine cameo ware, made by casing colored glass objects with a layer of another color, and cutting away so much of the second layer as to leave the design in delicate relief; the filigree ware, the aventurine vases, the frosted ware, the crackle ware, the spun and woven glass, and a hundred other beautiful and ingenious varieties of glass work, all have stories of wonderful interest to tell, had we but time to question them. In its employment for utilitarian purposes as well, glass has shown itself quite as adaptable as in the ornamental arts. Every day brings so many propositions for the application of the material to some new purpose, such as for fence posts, railroad sleepers and the like, that I

may safely leave the completion of the list to your imagination. Of the use of glass in the construction of houses, you have all probably received some hint, as well as what the persons who live in such houses should not do.

THE PARTICLE IN THE ROTATING TUBE.

BY IRVING P. CHURCH, C.E., Cornell University.

It is hardly necessary to lay stress on the importance of inculcating and vindicating correct principles of mechanics in a journal of this character, and I therefore venture a brief response to Mr. Frizell's article, in the recent July number (pp. 67–71), in the hope that, now he has conceded one term of his formula to be erroneous, two simple tests may be conclusive in convincing him that the whole expression is founded on a fallacy, even in its latest form.

Reduced to its "lowest terms," the problem is this: A tube of small uniform bore, with its axis in one plane and of any form (*i. e.*, any smooth curve), is kept rotating with a constant angular velocity, ω, about a vertical axis C, perpendicular to the plane of the axis of the tube. A small particle, nearly fitting the tube and of mass $= M$, is free to move along it, without friction, subject to no dynamic influences save from the tube and its motion; (we neglect gravity since motion is confined to a horizontal plane). If, in passing a certain point, 1, of the tube, at a distance r_1 from C, the velocity of the particle along the tube (*i. e.*, its relative velocity) is c_1, it is required to find its relative velocity c_2 when it reaches some other point, 2, of the tube, having given ω, c_1, r_1, r_2, and the form of the tube (if necessary) between 1 and 2.

Weisbach's solution of the above is given in the equation

$$M\frac{c_2^2}{2} - M\frac{c_1^2}{2} = \int_1^2 [\omega^2 M r]\, d r \qquad (1)$$

i. e.,

$$c_2^2 - c_1^2 = \omega^2 r_2^2 - \omega^2 r_1^2, \qquad (W)$$

his claim being that the change in relative velocity is solely due to a "centrifugal force" $\omega^2 M r$ (where r is the variable distance of the particle from C).

Now eq. (W) is the correct solution, as can be proved in the most detailed and rigorous manner (see p. 381, J. F. I., for May, 1887), but Weisbach's claim taken literally, as it would be by a student, is absurd, since the only force acting on the particle is the pressure of the side of the tube against it; while, if he is understood as asking us to grant that "the change in relative velocity is the same *as if* solely due," etc., he is practically begging the question, since few could be expected to give immediate assent to so sweeping an assumption, to yield which, virtually yields all.

Mr. Frizell's method of solution is identical with Weisbach's in assuming that a certain "centrifugal force" is the sole cause of the change in relative velocity, and is open to the same objection of making too long a stride at the outset. *His* "centrifugal force" is $[\omega - \omega']^2 M r$, (p. 68, of recent July number; an alteration from what he first maintained, on p. 31 of J. F. I., for July, 1884, now conceded to have been in part erroneous). Here ω', measured in a direction opposite to that of ω, is the angular velocity (about C) of M relatively to the tube, so that $\omega - \omega'$ is the absolute angular velocity of the particle at any instant. If w denotes the absolute velocity of the particle at any instant, and $90^\circ - \alpha$, the angle between w and r, we may write,

$$\omega - \omega' = \frac{w \cos \alpha}{r} \tag{2}$$

and hence, according to Mr. Frizell,

$$\frac{M c_2^2}{2} - \frac{M c_1^2}{2} = M \int_1^2 \frac{w^2 \cos^2 \alpha}{r} d r \tag{F}$$

which cannot be integrated unless w and α are known functions of r.

If "experiment is the court of last resort in disputed physical questions," let us be careful, in testing eqs. (W) and (F), to employ experiments, real or ideal, the facts in which are known with the greatest precision, and of such a nature that the blame of the slightest discrepancy between fact and formula must plainly be laid at the door of the "centrifugal term."

[The weak point in Mr. Frizell's claim that the Lowell experiments sustain his version of the "centrifugal term" in the "common theory" of turbines, is that, in making all discrepancies between experiment and formula chargeable to the "centrifugal

term," he not only calls upon us to grant his allowances for friction and the assumption of a five per cent. contraction at wheel-entrance to be correct, but virtually demands that the hypothesis of "laminated flow," which underlies the "common theory," shall be considered as very closely realized; whereas this supposition, which vastly simplifies hydraulic problems (and without which, in fact, we could hardly theorize at all in some instances), is well known to be only an approximation, which, in extreme cases (like Venturi's tube, for example), gives results differing by more than fifty or sixty per cent. from the truth, and requires corrective coefficients accordingly. It is therefore useless, in experiments with turbines of narrow crowns, to look for the effect of the "centrifugal term," entirely unmasked by other influences. Hence the necessity, in this purely theoretical problem of the tube and particle, of choosing test experiments involving but a single particle (instead of millions), moving without friction (and no one will deny that the results claimed would be precisely realized if friction could be entirely obviated).]

First Test.—Suppose that between the points 1 and 2 the tube is so shaped that the particle traverses it without contact with either side, and hence experiences no constraint from it whatever (horizontally). (That is, there need be no tube at all, but simply a smooth horizontal supporting surface between 1 and 2.) Its absolute path, then, between 1 and 2, must be *a straight line described with some constant* (absolute) *velocity* w (by Newton's first law of motion). Let the perpendicular distance from C to this straight line be a. Then

$$r_1 \cos \alpha_1 = a, \text{ and } r_2 \cos \alpha_2 = a; \tag{3}$$

while, from the two parallelograms of velocities,

$$c_1^2 = w^2 + \omega^2 r_1^2 - 2 w \omega r_1 \cos \alpha_1 \tag{4}$$

$$c_2^2 = w^2 + \omega^2 r_2^2 - 2 w \omega r_2 \cos \alpha_2 \tag{5}$$

By subtracting (4) from (5), noting from (3) that

$$r_1 \cos \alpha_1 = r_2 \cos \alpha_2,$$

we obtain (as an experimental fact)

$$c_2^2 - c_1^2 = \omega^2 r_2^2 - \omega^2 r_1^2 \tag{6}$$

which is identical with what eq. (W) gives for this case (and for

every case); while eq. (F) is totally at variance with (6). For, with w constant and $\cos \alpha = a \div r$, [eq. (3)], we have

$$\int_1^2 \frac{w^2 \cos^2 \alpha}{r} \, d\, r = w^2 a^2 \int_1^2 \frac{d\, r}{r^3} = \frac{w^2 a^2}{2} \left[\frac{1}{r_1^{\,2}} - \frac{1}{r_2^{\,2}} \right];$$

i. e., introducing ω, for comparison, (F) becomes

$$c_2^{\,2} - c_1^{\,2} = \frac{w^2 a^2}{\omega^2 r_1^{\,2} r_2^{\,2}} \left(\omega^2 r_2^{\,2} - \omega^2 r_1^{\,2} \right), \qquad \text{(F')}$$

the magnitude of the discrepancy depending on numerical data. Eq. (F) is therefore manifestly erroneous.

Second Test.—Suppose ω is made zero, *i. e.*, the tube to remain stationary, then the absolute is identical with the relative velocity at any point. For a frictionless stationary tube, it is an experimental fact that the absolute velocity remains constant, *i. e.*,

$$c_2 = c_1 \qquad (7)$$

Now eq. (W) with $\omega = 0$, gives $c_2 = c_1$, in exact accordance with fact whereas eq. (F), which does not contain ω, would assert that

$$c_2 > c_1 \qquad \text{(F'')}$$

and is again shown to be fallacious.

It is noteworthy in this second test that, since the tube is at rest, the position of the axis C can have no possible bearing on the result, a fact which confirms eq. (W) [from which r_1 and r_2 drop out], while according to eq. (F)

$c_2^{\,2} = c_1^{\,2} +$ a quantity dependent on the location of C; which is a manifest absurdity.

Another *reductio ad absurdum*, similar to the last, may be brought out by the following question, which I would like to propound to Mr. Frizell (and which is suggested by the circumstance that Mr. Frizell's "centrifugal term" does not reduce to zero when the Tremont turbine is held fast, at full gate, each wheel channel then becoming a stationary short pipe discharging water under a certain head):

If a short, straight pipe is fixed at an acute (horizontal) angle in the vertical side of a large stationary tank holding water, where should the vertical axis be taken in forming the "centrifugal term" which Mr. F.'s ideas compel him to incorporate in the formula for the discharge of this pipe?

It is, of course, true, as Mr. Frizell says, that if a body of mass M be compelled by a guide to follow a circular path of radius r, and has a velocity v in that path, then a centrifugal force of

$$\frac{M v^2}{r}$$

is developed [by which we are to understand, with Rankine, that the guide sustains an outward (centrifugal) pressure of the above amount, and the body an equal inward (centripetal) pressure]. But if the body (like a water particle in a turbine) is *not* following this circular path, but *some other* (*absolute*) *path* (whose direction and sharpness of curvature are not mentioned, nor the absolute velocity w of the body in it), the assertion (of Mr. F.) that a "centrifugal force" of

$$\frac{M v^2}{r}$$

is developed must certainly be taken *cum magno grano salis,* even if v *does* represent the component of w perpendicular to r (the other component being along r).

The reason for avoiding the phrase that an angular velocity is so many *feet* per second is this, that it is confusing to a student to tell him that while a *linear velocity* of two feet per second may also be stated as twenty-four inches per second, an *angular velocity* of two feet per second is not twenty-four inches per second, but two inches per second, or two miles per second, *i. e.*, the number 2 (so long as the second is the unit of time) is sufficient, and the employment of any unit of length is as superfluous as the use of a unit of volume or weight.

Niagara Falls, N. Y., July 28, 1887.

OBITUARY.

PLINY EARLE CHASE.

PLINY EARLE CHASE, a distinguished member of the FRANKLIN INSTITUTE, and acting President of Haverford College, died on Friday, December 17, 1886, at his home on the college grounds. PLINY EARLE CHASE, the oldest son of Anthony and Lydia Earle Chase, was born at Worcester, Mass., August 18, 1820, and consequently, at the time of his death, was in his sixty-seventh year. He was graduated at Harvard College in 1839, and for some time thereafter devoted himself to teaching. His first engagement in this career was as the principal of a district school in Leicester, Mass.; subsequently he was appointed to the principalship of a school in Worcester. He came to Philadelphia, which was destined to be his future home, in 1841, and engaged in the same avocation ; first, in the Friends' select school, and later in a private school of his own. On account of impaired health, he abandoned teaching in 1848, and was engaged in mercantile pursuits for some ten or twelve years. During this period, he carried on the stove and foundry business, under the firm names of North, Harrison & Co., North, Harrison & Chase, North, Chase & North, and Chase, Sharp & Thompson, the late John Edgar Thompson, President of the Pennsylvania Railroad Company, being a silent partner in the last-named firm.

In 1861, he again returned to teaching, succeeding the late Prof. C. D. Cleveland in the ownership of a prominent school for young ladies, at 903 Clinton Street, in Philadelphia.

In 1871, he received the appointment of Professor of Natural Science in Haverford College. He was afterwards transferred to the Chair of Philosophy and Logic, and at the time of his death, and for nearly a year prior thereto, he was the acting President of that institution. He also occupied temporarily the post of Professor in the University of Pennsylvania, made vacant by the death of Prof. John F. Frazer ; and that of Lecturer on Psychology and Logic in Bryn Mawr College.

PROF. CHASE'S relations with the INSTITUTE date from 1851, in which year he became a member. He was elected a member

of the Board of Managers in 1864, and of the Committee on Publications in 1869, and served continuously in both offices until his death. He was frequently called upon to assist in the work of instruction, and his name appears on a number of the annual programmes as a lecturer, upon subjects relating, principally, to astronomy and meteorology. For a number of years, also, he prepared the scientific notes which have added much to the general interest of the JOURNAL, to which he was likewise an occasional contributor.

PROF. CHASE devoted much time to philosophical, philological and physical studies, and was esteemed universally as a scholar of extraordinary versatility, and as an original thinker of the highest order. The thoroughness and extent of his scientific work are shown by his numerous published writings, which include about 120 titles in the register of papers published in the *Transactions and Proceedings of the American Philosophical Society;* about fifteen articles in other periodicals, principally the *American Journal of Science* and the JOURNAL OF THE FRANKLIN INSTITUTE, and several small independent works, the last of which is his *Treatise on Meteorology.* This extensive list of publications bears evidence of the intense mental activity of his life. In the beginning, and occasionally all through the list of publications, we see his interest in the mysteries of the origin of language; his studies in Sanskrit, Chinese and Indo-European roots and analogues, were followed by a Copto-Egyptian vocabulary; but with slight exceptions, all of his works since those first years have been confined to problems in Cosmic, Terrestrial and Molecular Physics. In these departments, it has been not so much observation and experiment, but rather the search after theoretical relations and the deductive establishment of new principles that has chained his attention. It would seem as though to his mind the operations of Nature were conducted by the same laws, whether on the smallest or on the largest scale. Thus, from the motions in a small mass of resisting material, he deduced the sun's mass and distance; from the cosmic relations of gravitation and inertia in the solar system, he deduced the velocity of light; by studying the loci of planetary aggregation under the perturbing influence of Neptune and Jupiter on the asteroids, he predicted the location of an unknown planet.

In meteorology, his work includes the distribution of heat and barometric pressure; the polarization of sky-light; the periodicity in rainfall at Philadelphia and over the world; the effect of cosmic influences on meteorology; the relation of the aurora to rainfall; the laws of cyclonic and anti-cyclonic movements. In his meteorology, he has brought together a number of the more recent results of studies, especially those bearing on the local indications and general predictions of the weather, and, we are informed, has educated his scholars in a method of weather prediction based upon his tidal and harmonic tables. Unfortunately, however, these methods of prediction have in them too much that is empirical to justify their adoption by ordinary meteorologists. To the general reader his works undoubtedly seem imbued with the spirit of Kepler, and with his patience also, who toiled through innumerable speculative computations to eventually fall, most fortunately, upon the three laws that served Sir Isaac Newton as tests of the truth of his own brilliant inductions.

The general impression which the work of Prof. Chase has made upon his scientific contemporaries is perhaps fairly expressed in the following abstract from a letter received by the Committee from a scientific man of eminent position and reputation, to whom the Committee had applied for data to aid in the preparation of the foregoing inadequate memorial:

"I comply with your request to contribute a few words upon Chase's scientific contributions, although I am no more worthy to pass judgment upon them than are the others to whom you have applied. I should be glad to examine his works in detail, more thoroughly, but their enormous extent utterly forbids my attempting it. I have in years past frequently read them with amazement, not understanding at all the logical process by which he arrived at his conclusions, many of which, however, served to confirm a principle recognized by mathematicians, namely, that a given law expressed in one set of terms will be found to be applicable to one class of natural phenomena, while the same law or equation will apply to another class of phenomena, if we merely attribute new and appropriate interpretations to the constants or data entering into the equation. On this principle Drummond, of late years, has developed his work on natural law in the spiritual world, and this seems to be the fruitful idea that pervaded the whole of Prof. Chase's publications."

In 1864, the American Philosophical Society, of which PROF. CHASE was one of the most active members, awarded him the "Magellanic Gold Medal," for his paper on "The Numerical Relations of Gravity and Magnetism."

In character, PROF. CHASE was one of the most lovable of men, and the following tribute to his memory from a brother professor will be warmly endorsed by all who knew him well:

"With the widest attainments in the field of knowledge, he preserved the greatest simplicity and humility. He was always ready to hear, and weigh fairly the opinions of others; and, when necessary to maintain his own, it was always done with modesty, courtesy and kindness. Retiring in his disposition, it was often difficult to draw forth an opinion from him. He was a member of the Society of Friends, and a thorough believer in its principles. The simplicity of his Christian faith and the beauty of his life must long be remembered by his friends." THE COMMITTEE ON PUBLICATIONS.

BOOK NOTICES.

THE SEPARATE SYSTEM OF SEWERAGE. By Cady Staley and Geo. S. Pierson, C. E. New York: D. Van Nostrand.

The authors of this work commence by calling attention to the imperative need of some system of sewerage as soon as population becomes aggregated within a limited area, the existing conditions being forcibly stated in the following passage: "An examination into the sanitary condition of a majority of our older cities and villages will show the great need of some kind of sewerage. Many of them have never taken any measures to rid themselves of the necessary accumulations of filth incident to a considerable population. For generation after generation, the refuse which should be removed from the dwellings, has been flung upon the surface of the ground, or into cess-pools, where the putrefying mass poisons the air and appeals in more ways than one for a remedy."

After pointing out the evils of the Combined System, the advantages of the Separate System are explained, and numerous tables are given, showing the rate of flow of liquids through pipes, etc., which indicate the scientific knowledge requisite to properly remove the liquid wastes from a given area.

The applicability of the Separate System is set forth as follows: "The introduction of the Separate System marks an important era in the development of sanitary drainage, recognizing, as no other system has, the prime importance of an early removal of household and industrial wastes, which are the main factors in soil pollution. That it will best meet the requirements of all large and densely populated cities (economy considered), is not probable. That, under competent advice, it can meet the requirements of *house*

drainage more perfectly in *any city* than the Combined System, cannot be denied. It is peculiarly adapted to many of the numerous smaller cities, which have been practically debarred from sewerage by its cost, and to outlying portions of larger ones. Its comparatively small cost permits an early and general extension, and the removal of domestic wastes before the soil has become saturated with them beyond a reasonable hope of purification."

This permanent pollution of the soil is a matter of the greatest importance, and the dangerous consequences are seen in the instances where the opening of the streets for the laying of gas or water pipes has been followed by sickness, in the adjacent houses, of a character that indicated that the emanations from the exposed earth were the cause.

The work is illustrated by cuts, showing the various flush tanks, etc., and also showing the practical details of laying sewer pipe. Forms for specifications are given, which render it a complete hand-book for the practical engineer.

W. B. C.

Boiler-making for Boiler-makers. W. H. Ford, M.E. New York: John Wiley & Sons. 1887.

It is safe to say that this little book contains in clearly stated language more practical information than can be found in any other on its subject approaching its size.

It is addressed to practical workmen, and employs only terms and illustrations easily understood by them.

Whilst it embraces every point of importance in the art, every detail is promptly accessible through an unusually full and exact index.

The modest preface of three small pages shows the intention of the author to supply a real need of the craft for a concise, practical treatise within the easy comprehension of the workmen in the business. The book meets and fills the want. Keeping in mind that the author announces, "*this book is entirely devoted to the workshop*," there is very little that could profitably be added to it, unless perhaps a reduction of its few formulæ to expressions in purely arithmetical rules and terms with examples of their practical application for the benefit of those who were never taught or trained to appreciate anything resembling algebra, but who yet work safely and confidently with plain arabic figures. There are many men of such attainments among practical boiler-makers, and such an additional feature might be highly appreciated by them. Altogether, there are few books which present such full and exact knowledge, concisely stated in such a clear and readily accessible shape.

S. L. W.

Grimshaw's Pump Catechism. By R. Grimshaw, M.E. New York: Practical Publishing Company. 1887.

This book gives in compact shape much information that is directly to the point on the subject, which, when needed, is often required with an urgency that does not permit of a search through the many publications in which it is scattered.

It gives a plain and distinct showing of the construction and operation of the numerous hydraulic machines in general use, such as could only be had by the perusal of hundreds of advertising pamphlets and catalogues of rival manufacturers, the conflicting claims of many of which are confusing if not suggestive of unreliability, and is valuable in that it informs the reader what he may not expect to do with pumps as well as showing how to use them and the principles which govern their operation.

To the many manufacturers and others whose operations compel the use of pumps, such a book is a great benefit, its language is entirely within the comprehension of the class of persons employed about such work. In short it is a valuable addition to the author's series of practical popular books. S. L. W.

SCIENTIFIC NOTES AND COMMENTS.

PHYSICS.

SYSTEMS OF ELECTRICAL DISTRIBUTION BY ALTERNATING CURRENTS AND TRANSFORMERS.—In the London *Electrical Review*, March 18th to April 22, 1887, there is published a series of interesting articles by Rankin Kennedy, on "Electrical Distribution by Alternating Currents and Transformers," reprinted in the *Electrician and Electrical Engineer*, New York.

In a brief history of such systems, obtained apparently wholly from the record of English patents, he cites, as the earliest record, a patent to Jablochkoff, in 1877, for using induction coils with alternating or intermittent currents for distributing lamps. The transformers were placed in series, and were apparently used for obtaining a higher from a lower potential, as in the ordinary Rhumkorff coil. The next in order are the patents to Harrison, 3,470, of 1878, Bright, 4,219, of 1878, and Edwards and Normandy, 4,611, of 1878, the latter of which he says is important, as it is a clear and distinct description of a complete system. The coils are, in all of these, placed in series, and no mention seems to be made of converting high into low tension, other than a statement in Bright's patent that "the size and length of the primary wire and secondary wire coils are adapted to the number of lights employed." The next patent, No. 5,183, of 1878, to J. B. Fuller, of New York, seems to be the first to mention a regulating device by varying "the magnetic field." The next is that of De Meritens, 5,257, of 1878, which makes no advance on prior inventions. No other patents are mentioned prior to 1882, to Gaulard & Gibbs, which, he says, has "all the elements arranged precisely as in the systems patented or described by Jablochkoff, Edwards and Normandy, Bright and De Meritens." In another patent to Gaulard and Gibbs, 1884, he says that an attempt is made to prove that the transformers connected in series are self-regulating. The writer then states that in June, 1883, he, for the first time, described the actual effects of counter E. M. F., and stated that when the transformers are connected in parallel they became beautifully self-regulating. After he made this statement, Messrs. Zipernowski and Déri, of Buda Pesth, applied for a patent (No. 3,379, March 16, 1885,) for placing the transformers in parallel. He considers this to have been a great step in the

history of such systems, and claims that "no self-regulating transformer has been invented up till this date, which will give constant E. M. F. in the secondary circuit, with constant current in the primary circuit;" that is, when connected in series. He states the law that "as the current in the secondary circuit increases, the counter E. M. F. of the primary *decreases*, and, therefore, the current in the primary circuit increases," which law, he says, renders them perfectly adaptable to connection in parallel order to a supply at constant E. M. F. He states that the law is similar in every respect to that for motors in a continuous current circuit.

The next patent is that of Ferranti, No. 15,251, of December, 1885, the claims of which he gives in full, "to show what can be put into a British patent." It contains transformers in parallel and other accessory devices. The next is that of Siemens and Halske, No. 16,038, of 1885. The United States Gaulard and Gibbs patent is dated October 26, 1886.

In the patent of Edwards and Normandy, as well as in that of Ferranti, it was proposed to employ a converter for raising the potential of the (low potential) dynamo current, and from this converter to supply the other converters in the system. Such a system was constructed by the writer (Kennedy), in 1886, with an alleged loss of less than two per cent. in the first transformers, in which the dynamo potential of fifty volts was raised to 2,000.

In his history of such transformers, the writer does not offer any records of the invention of one of the chief and essential parts of such transformers, that of reversing the function of the well-known induction coils, and thereby converting a high tension into a low tension current of great quantity. He states at the end of his article, merely that "there is an impression abroad" (by which he undoubtedly refers to the United States) ' that the mere employment of the fine wire as primary and the thick as secondary in an induction coil was a great invention made only recently, I am certain that this has been tried by innumerable experimenters during the past thirty years." It is to be regretted that he did not furnish some proof of at least one of the "innumerable" cases. In the absence of such proof, it may be of interest to quote here, from the JOURNAL OF THE FRANKLIN INSTITUTE, May, 1887, p. 357, in which Prof. Elihu Thomson, in referring to some lectures which he had delivered at the INSTITUTE ten years ago, says: "After showing the induction coil as so used, I reversed the process and passed high potential discharges from a charged battery of leyden jars, through the fine wire coils of the induction coil, and received currents of low potential but of great volume from the coarse wire of the coil. By putting a low resistance galvanometer in the circuit of the coarse wire, known ordinarily as the primary, a strong deflection of the index of the galvanometer took place, and upon bringing the ends of the coarse wire coil together in slight contact, a bright green flash took place at every leyden jar discharge through the fine wire."

It may also be of interest, in this connection, that Alexander Bernstein, of Boston, applied for a U. S. patent in January, 1883, for such a system, but it was rejected, on the ground that "it is not apparent how applicant in any case can get more quantity from a secondary coil than he has in his primary," by which, we presume, quantity of current is meant, as distinguished from quantity of energy.

In the history of the transformers themselves, Kennedy begins with the form invented by Faraday, in 1835, which consists of an iron-ring core, the two halves of which are wound, respectively, with the two wires, resembling, therefore, a Gramme ring without commutator, and having its endless winding cut at two points, thus forming two separate coils. This form is a very good one, as the closed magnetic circuit of the lines of force is composed entirely of iron; although much older than the simple magnet form used in the Rhumkorff coil (1842) or in the Gaulard and Gibbs system, it is much better than the latter, as they have open iron magnetic circuits, instead of having the whole magnetic circuit of iron.

He goes on to describe the "evolution" of the modern transformer from that of Faraday by the inventions of Kennedy, 1883; Ferranti, 1885; Stanley 1886; Dick and Kennedy, 1886, and Zipernowski and Déri, 1885, the best and "most novel" development being in the latter, which is the "highest class of transformer, reducing the magnetic resistance to a very small value." This invention consists in forming the two conductors into what might be termed a core of ring form and then overwinding them with iron wire; in fact, it is the transformer of Faraday turned inside out—instead of the conductors enclosing the iron, the iron encloses the conductors. Such transformers exhibit no external (wasted) magnetism, and, therefore, represent the greatest economy of magnetism.

In this history of the "evolution" of the transformer, the writer appears to neglect dates, as the alleged improvements of Ferranti, Stanley and Dick and Kennedy appear to have been invented after the Zipernowski and Déri, and were, therefore, steps in a backward direction. Furthermore, the writer does not seem to be aware of the fact that in the English patent of Staite, No. 12,212, of 1848, an induction coil precisely like that of Zipernowski and Déri is distinctly shown, described and illustrated, the only unessential difference, considering it as an inductor merely, is that it contained only one coil instead of two, the object of it being to increase the self-induction of that one coil. The placing of the iron around the ring-form conductor is, therefore, older perhaps than some of the later inventors of inferior devices.

Kennedy's later form called a sub-divided transformer, appears to be a novel and ingenious improvement of the Zipernowski and Déri or Staite sheathed coil, as any or all of the sub-divided coils are accessible and may be removed or cut out in case of damage without unwinding all the iron wire covering.

After describing a few more of the latest forms of transformers, he concludes his history by saying "the introduction of this system in America has created a somewhat amusing flutter among the electrical fraternity." We agree with him, and recall here a statement made in the FRANKLIN INSTITUTE JOURNAL some time ago, regarding these systems: "It is not very creditable to the Americans to bring before the public as new, a system which has been known and used in Europe for a number of years past."

He concludes his interesting articles with rules of construction, among which are the following: "To be self-regulating for supplying a constant potential and varying quantities of current, the transformers should have the

primary coils connected in a multiple arc system in which the mains have a constant potential. The lengths of wires must be found for self-regulation according to the laws:

(1.) When no work is being done in the secondary circuit, there should be no current in the primary circuit; that is, the counter pressure in the primary must be equal to the primary pressure, or very nearly equal thereto.

(2.) As the current increases in the secondary circuit, the current in the primary should increase proportionally in the ratio of the transformer; that is, the counter pressure in the primary should fall as work is increased in the secondary.

"All transformers working in one system must be constructed accurately with a fixed ratio."

Rule 1.—To find the lengths of the primary and secondary conductors in a transformer for reducing from a high to a low pressure, divide the primary pressure by the coefficient of induction in the primary circuit (that is, by the induction in volts per foot of wire), this will give the length in feet of wire for primary.

Rule 2.—Divide the secondary pressure by the coefficient of induction in volts per foot, this will give length in feet of wire for secondary.

In the best transformers working with about 200 alternations per second this coefficient is very high and averages about two volts per foot, and that may be taken as the figure for use in the above rules; for example:

Given a primary pressure of 1,200 volts, and the required secondary pressure given as 80 volts, what lengths of primary and secondary wire should be employed?

$$\text{Answer,} \quad \frac{1{,}200}{2} = 600 \text{ feet for primary,} \quad \frac{80}{2} = 40 \text{ feet for secondary.}$$

These lengths do not include wire not under induction.

If the alternations are not so rapid this coefficient becomes less. It might be argued from this fact that as it is perfectly easy to make the alternations very much more rapid than 200 per second, that, say 500 or 1,000 alternations should be adopted. There is a limit to that; as the alternations increase in number per minute the losses in the transformer increase, and that fact often accounts for different efficiencies being found for the same transformer by different experimenters—the one, perhaps, has employed the alternations for which the transformer was calculated, the other has employed slower alternations and therefore no agreement could be come to. It is, therefore, necessary to ascertain by experiment the coefficient of induction for each type of transformer at a given alternation rate. In transformers with long magnetic circuits, common to all the coils of wire, the coefficient will not be higher than 1·5, and often falls as low as one foot per volt.

Practically this coefficient may be found by taking the given alternating pressure having the given rate of alternations through an approximately correct length of primary wire on the transformer, say a length calculated at two volts per foot (the secondary circuit being open or not yet wound on), and measuring the current produced through this length. If no measurable current

is observable then that is the correct length; there should be some current, of course, say, perhaps, two per cent. of the maximum current. If the coefficient of induction is less than that calculated upon, a considerable current passes in the primary when no secondary current is flowing. The primary wire must, in that case, be increased in length until this initial current disappears, or becomes so small that it is of no account, and then the length of secondary can be calculated by the rule.

Rule 3.—The ratio of a transformer is found by dividing the primary pressure by the secondary pressure, thus in my previous example:

$$\frac{1,200}{80} = 15.$$

15 would be said to be the ratio of that transformer, and thus

	Primary.		Secondary.
The pressures are to each other as	15	to	1
The lengths of conductors to each other as	15	"	1
The cross-section of conductors to each other as	1	"	15
The currents to each other as	1	"	15

The iron conductor of magnetic force should not at any time be anywhere near half saturation; it should therefore have large cross sections, and data for calculating its size may be obtained from dynamo builders."

In another article on induction coils, by Gisbert Kapp, in *Industries*, reprinted in the London *Electrical Review*, April 22 and 29, 1887, pages 366 and 390, the writer makes the following statements:

"A person having no theoretical acquaintance with the behavior of alternating currents, would compute the rate of doing work" (*i. e.*, the power) "in the circuit by simply measuring the current (say, with a Siemens dynamometer), and the terminal pressure of the machine (say, with a Cardew's voltmeter), and multiply the two. The product he would, generalizing from his experience with continuous current machines, consider equal to the rate of doing work. But this would be completely wrong, because he would have neglected the time element."

"A ring-shaped inductor, wound in the Gramme fashion, that is, with the iron inside and the copper outside," (*i. e.*, Faraday's form,) "will not be as economical in weight of materials used as the inverse arrangement of copper coils inside and iron wire wound over them to form the outside" (*i. e.*, Zipernowski and Déri, or Staite form).

He shows from theoretical deductions that transformers connected in series cannot be made self-regulating for giving a constant potential in the secondary, unless approximately when the amount of iron is small and over-saturated. That such a transformer may have its secondary coil short-circuited without the current in it becoming very great. That the characteristics of transformers in parallel is a straight line, and that it therefore is perfectly self-regulating for constant potential. C. H.

CHEMISTRY.

PHOTOGRAPHY BY VITAL PHOSPHORESCENCE.—Dr. Jno. Vansant publishes the following observations in the St. Louis *Photographer*, July, 1887: Some months ago, there was published in several scientific journals,* an article in which I showed how excellent photographic positive prints, on glass or paper, could be made from an ordinary negative by means of the transformed or "stored-up" radiant energy—the phosphorescent luminosity—of certain inorganic substances, especially particular sulphides of calcium and strontium.

Many organic substances also, as is well known, possess this property of storing-up, so to speak, and afterwards emitting, as more or less luminous rays, the radiations to which they have been exposed. Crystallized carbon, in form of the diamond, and white paper may be cited as illustrations of this class. A photographic latent image on a bromide of silver surface, capable of being developed, can easily be produced by bringing into *contact*, for an hour or so, in the dark, such a sensitive surface, and an engraving, or some ordinary printing, on white paper which has been just previously exposed for some minutes to the direct rays of the sun.

But I have now to call attention to the curious fact that the kind of light given out by certain *animal organs*, and which evidently in its causation has some close relation to the nervous system and vitality of the animal, and belongs to a different class of phenomena from the phosphorescence above mentioned, can also bring about incipient decomposition in a haloid salt of silver. Moreover, it can do this through a sheet of glass of the usual thickness used for photographic negatives, and, consequently, there is a possibility of producing by such light photographic positive prints.

The following experiment, copied from my notes, proves this:

June 8, 1887.—This evening, just after dark, I took about a dozen fire-flies (*Lampyris corusca*), which had been captured a few minutes before on the lawn, and enclosed them in a wide-mouthed vial of some three ounce capacity, having a piece of fine white bobinet (such as is used for ladies' veils) stretched over its mouth in place of a stopper. Enclosed thus, they would frequently emit the momentary flashes of greenish-tinted yellow light for which they are remarkable, though usually only one insect at the same time would flash. Every few seconds one or another would emit its light for a period, which I estimated to average in each case about one-half of a second, and the frequency of the emissions could be increased by gently shaking the vial. When not flashing, the under surface of the three posterior segments of the fire-fly's abdomen, from which the light came, was scarcely at all luminous, but was simply of a bright yellow color. The flashing was plainly under the control of the insect, like its muscular movements. These fire-flies are rather less than three-quarters of an inch long, and the segments which become luminous have, altogether, an area of only about one-eighth

* *Scientific American Supplement*, February 12, 1887; Philadelphia *Photographer*, April 16, 1887.

of an inch square. The flash is, however, quite bright, so much so that fine print can be easily seen when held close to it.

Repairing to my dark closet with the vial of fire-flies, I placed it to one side, under cover, whilst I arranged and clamped a very sensitive gelatino-bromide of silver dry plate beneath an ordinary negative picture of a landscape on glass, as for contact printing.

The vial of insects was then inverted over the back of the negative, so that only the fine meshes of the bobinet and the glass of the negative with its gelatine film intervened between the fire-fly's light and the sensitive bromide plate. I counted the flashes, occasionally shaking the vial and sliding it over the negative, till fifty flashes had occurred.

The vial was then removed, the sensitive plate separated from the negative, and an attempt made to develop the latent image, if any existed. Alkaline solution of pyrogalol was used, and, in a few minutes, I had the pleasure of seeing a well-marked positive image of the negative picture appear, the plate being somewhat yellow stained, as if from too long an exposure. This was fixed in the usual way with sod. hypo sulp., and is now in my possession—probably the first picture ever *produced by the light emitted from a living animal organism.*

U. S. MARINE HOSPITAL,
St. Louis, Mo., June 10, 1887.

DIAMIDE OR HYDRAZINE. Theodor Curtius, *Berliner Berichte*, **20**, 1632.)—When diazoacetic ether is treated with hot concentrated potassium hydroxide solution, the potassium salt of a new diazo-fatty acid is obtained as large yellow crystals. This salt differs from the hitherto described diazo-compounds in that when treated with mineral acids it is not decomposed with elimination of nitrogen, but the free diazo-acid separates in golden-yellow crystalline plates.

If the aqueous solution of this acid is digested for a short time with warm and very dilute sulphuric acid, it becomes decolorized, and, after cooling, a colorless substance separates in handsome crystals. This substance is the sulphate of the long sought diamide or hydrazine $(NH^2)^2$, and, owing to its slight solubility in water, can easily be obtained perfectly pure. It crystallizes in anhydrous clino-basic tables, very little soluble in cold, easily in hot water; insoluble in alcohol.

Free diamide is disengaged as a gas when one of its salts is warmed with the solution of an alkaline hydroxide. When much diluted, it has scarcely any odor, but when strong, its odor is peculiar, yet unlike that of ammonia; it attacks the mucous membrane of the nose. It is very soluble in water; it at once changes red litmus paper to blue, and when not too much diluted with air, produces white fumes on contact with vapors of hydrochloric acid.

It is a powerful reducing agent, reducing Fehling's solution and ammoniacal silver nitrate in the cold, and, when warm, separating the copper in the form of a mirror. Even neutral cupric sulphate is reduced, with the precipitation of a dense deposit of cuprous oxide. The solutions of hydrazine salts are decomposed on contact with nitrites with violent effervescence.

W. H. G.

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REPORT ON THE CHEMICAL COMPOSITION OF NATURAL GAS.

Made by PROF. FRANCIS C. PHILLIPS, of the Western University, Allegheny, for the Geological Survey of Pennsylvania.

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COLLECTION OF SAMPLES.

Glass vessels, having a capacity of 250 to 400 cubic centimetres, were carefully dried by a current of warm air, and in order to obtain the gas as nearly as possible free from moisture, the following method was employed.

Glacial phosphoric acid, partially cooled from fusion, was drawn out into fine threads. A considerable number of such threads, in short pieces, could be pushed through the glass stop-cocks, by

which the vessels were closed, and left in the vessels, which were then ready for the reception of gas samples. It is of importance to state that these vessels had been long in use for the same purpose, and had been proved to be air-tight by thorough and repeated tests. In collecting the samples, several of these glass cylinders were connected in a series with the well or main by a short rubber hose, and gas allowed to flow for twenty minutes through them all. The stop-cocks were then closed in such a manner as to leave a slight excess of gas pressure in each vessel.

The stop-cocks (which had previously been well-greased with a mixture of tallow and wax), were then wound over and completely covered by fine cord, so that each resembled a ball of cord. The capillary ends of the cylinders were then closed by short pieces of thick rubber hose plugged with glass rods. By this mode of wrapping all movement of the stop-cocks during transportation on railroads is prevented. The gas thus left in contact with the glacial phosphoric acid was gradually dried and ready for analysis on reaching the laboratory.

The common method of taking a gas sample in a glass cylinder having finely drawn out ends, which are to be sealed by a flame when the vessel is filled, is not applicable in the case of natural gas. The constant escape of gas about a gas well renders the use of a flame absolutely impossible on account of the danger of accident. Vessels closed by glass stop-cocks capable of holding a gas sample for many weeks without risk of leaking are now supplied by dealers.

METHOD OF ANALYSIS.

The determination of carbon and hydrogen existing in combustible form in the gas was conducted by combustion over oxide of copper in a porcelain tube, which was kept at a bright-red heat, and the resulting carbon dioxide and water, collected separately and weighed.

One of the glass cylinders, filled with gas at the well, was placed in a vertical position, and the temperature observed at intervals. When it was found that the temperature had remained constant for two hours, the lower stop-cock was opened for a moment to allow the excess of gas to escape and secure equilibrium between the pressure of the gas inside and that of the atmosphere. At the same time, the temperature and height of the barometer

were recorded. The glass cylinder was then connected with a porcelain tube containing oxide of copper, and already heated to intense redness in a furnace, and the gas forced out of the cylinder by dry mercury. As the gas escaped from the cylinder it was carried through the porcelain tube by a slow stream of nitrogen, previously dried by suitable means.

The gas was thus burned completely to carbon dioxide and water, which were collected and weighed by the usual methods, using a balance plainly sensitive to $\frac{1}{10000}$ gramme.

After the combustion, the glass cylinder was accurately calibrated by means of mercury at a known temperature, and thus was determined the exact volume of gas which had been burned.

As it appears possible under the conditions of the method that some nitrogen might undergo an oxidation, the water produced in the combustion of the gas was carefully tested, but in no case was the water found to have an acid reaction.

In the above described method are determined the weights of carbon and hydrogen per unit-volume of gas. In conducting the combustion, great care was taken to secure complete oxidation of the combustible constituents, and absorption of the products. For the absorption of the water, sulphuric acid of 1·71 specific gravity, followed by phosphoric anhydride, was used, and for the carbon dioxide a solution of caustic potash in glycerine.

For the determination of nitrogen, the following method was employed: A porcelain combustion tube containing oxide of copper was brought to a yellow heat, and a stream of carbon dioxide conducted through the tube until the last traces of air were expelled. The expulsion of the air was considered complete when it was found that the carbon dioxide escaping from the tube was wholly absorbed by a solution of caustic potash—100 cubic centimetres of such gas not leaving a visible quantity unabsorbed by the alkaline solution. Then, after the expulsion of the last traces of air, a quantity of natural gas (100 cubic centimetres were generally employed) was allowed to flow slowly into the stream of carbon dioxide as it entered the combustion tube. In this manner, the gas was burned and a mixture of nitrogen and carbon dioxide collected in an eudiometer or caustic potash solution. After the absorption of the carbon dioxide the volume of the residual nitrogen was measured. This nitrogen was carefully tested for

carbon dioxide, oxygen and carbon monoxide, and was frequently re-passed through the heated combustion tube and again measured, in order to secure the complete combustion of all hydrocarbons. This repetition demonstrated in all but one or two instances that the nitrogen was pure. It was found that with a sufficiently slow stream of gas the oxidation by the oxide of copper is easily rendered complete, although the rate of flow must be regulated with great care.

By the common eudiometric method of analysis no determination is more difficult than that of nitrogen when occurring in small quantities in admixture with hydrocarbons of the paraffin series. In the method above described large quantities of gas can be employed, and the results are accurate.

The determination of free oxygen in natural gas cannot well be made with the quantity of gas commonly at disposal. A test was made in every instance in about 100 cubic centimetres of gas, using an Elliott apparatus, and, as an absorbent, a solution of caustic soda and pyrogallic acid. In all cases the results were negative.

I have found it necessary to conduct the tests for oxygen at the wells, and this was done in the following manner:

A slow stream of gas was caused to flow (directly from the well or main) successively through solutions of caustic potash and pyrogallic acid for ten minutes, in order to expel dissolved air. Then by a simple contrivance the two fluids were mixed without interrupting the current of gas, which continued some time longer through the mixture. If the mixed fluids then exhibited a brown color, gradually increasing in depth, it was considered that the presence of oxygen was established.

The direct determination of free hydrogen has generally been considered a matter of such difficulty that in many published analyses its quantity has been estimated by a calculation based upon the total carbon and hydrogen contained in the gas. For the present purpose a direct determination seemed very desirable, and the process of Hempel has been used in the manner below described: 100 cubic centimetres of gas after the removal of carbon dioxide, were washed with a strong alcohol until the higher hydrocarbons—ethane, propane, etc.,—were removed; this was carried out in an Elliott apparatus having a water-jacket. Then the residual gas mixed with two or three times its volume of

air was passed over asbestos coated with thirty per cent. of palladium sponge, at a temperature of 90° C.

By this treatment the hydrogen alone is burnt, provided the higher paraffins, including ethane, are previously removed by washing with alcohol. From the contraction in volume, after passing the palladium, the proportion of free hydrogen is easily determined. The method is very accurate, when methane is the only hydrocarbon present. It is inaccurate in the presence of ethane and the higher members of the series, and when these are present the washing with alcohol must be long continued. As it is a matter of great difficulty to retain hydrogen, even by the help of the most carefully-ground stop-cocks, the tests for this element were made in all cases at once after the arrival of the samples in the laboratory.

The olefines as a group and carbon monoxide are much more easily determined in natural gas than the paraffins and free hydrogen. The olefines are quickly absorbed and removed by bromine water, and carbon monoxide by a solution of cuprous chloride. These reagents are used in the order named. Unfortunately, however, these fluids are likewise solvents, in less degree for the paraffins—ethane, propane, etc. Hence a gas perfectly free from olefines and carbon monoxide is liable, on being washed with the above-named fluids, to undergo a reduction in volume, leading to a wrong conclusion.

For the determination of these substances the following process was used, based on the solubility of both in a cuprous chloride solution. At the gas well a stream of gas was caused to bubble for two hours or more through 100 cubic centimetres of a solution of cuprous chloride. The solution was preserved for examination in the laboratory.

A quart flask, provided with a glass delivery tube and a funnel tube reaching to the bottom, was filled with boiled water and then the cuprous chloride, prepared as above described, was poured into the flask through the funnel tube. The flask was then heated to the boiling point, and the water caused to boil for three hours. A small quantity of gas was invariably collected from the cuprous chloride solution by this treatment.

The gas so collected was transferred to an Elliott apparatus and carefully tested for olefines and carbon monoxide by bromine

water and cuprous chloride solution. In this way the quantities of these two constituents in a very large quantity of gas could be collected in concentrated form, convenient for a qualitative test.

Carbon dioxide was determined by means of moist potash in an eudiometer over mercury, and also in the Elliott apparatus over water, by caustic potash solution. The latter method yields very correct results.

In addition to the determination carried out in the laboratory, the gas at the well was caused to pass in a slow stream through lime water. The stream of gas was made approximately the same by using the same delivery tube, depth of lime water and shape of containing vessel, and by counting the number of bubbles per minute, and then noting the rapidity with which the lime water became milky. For the detection of ammonia, the gas at the well was caused to bubble through 100 cubic centimetres of water, which had been carefully purified by distilling with addition of sulphuric acid and permanganate of potash. This water was afterwards tested by Nessler's solution, after the common method in use in the examination of drinking water for ammonia.

The presence of exceedingly minute traces of ammonia could thus be shown with great accuracy. As solid masses of ammonium carbonate are reported to have been thrown out from the pipes leading from gas wells in the Murrysville field, this test seemed very desirable.

In the statements of the results of analysis, all gas volumes are to be understood as "normal;" that is, the volumes observed under different conditions of temperature and pressure are all reduced to zero, Centigrade, and 760 millimetres mercury pressure, and where measured in a moist condition are calculated as dry.

The temperatures were all measured by one and the same thermometer, of which the error was known from a comparison with the Yale Observatory standard. This thermometer was made by Green, in New York, and is divided to $\frac{1}{10}$° C.

The barometer used was made by Hicks, and indicated by vernier, changes of $\frac{1}{1000}$ inch. The constant error of this barometer was ascertained by comparison with the standard barometer of the Signal Service Department, in Washington.

In all cases of gas measurements in eudiometers, the observations were made by means of a Grunow cathetometer, having a millimetre scale and reading easily to $\frac{1}{20}$ millimetre.

The etched scales upon the eudiometer tubes, as commonly supplied, are very often incorrect, both as regards uniformity and total length of scale, and are unsuited for accurate measurements of pressure or volumes. The glass cylinders containing the gas samples for combustion were calibrated at a temperature not differing by 1° C. from the temperature at which the gas was measured for analysis. In this way the calculation of errors due to expansion and contraction of the glass vessels was rendered unnecessary. This necessitated repeated calibrations after nearly every combustion.

In the calculation of the results of analyses, the following plan was adopted:

The percentage of carbon dioxide was determined volumetrically. Having failed to find carbon monoxide and olefines in any of the samples, they are necessarily left out of account in the calculation. Having found free hydrogen in only one of the gas samples, and here in traces (Speechley), it is also to be ignored in the calculations.

The quantities of carbon dioxide and water produced in the combustion of a known volume of gas were weighed. From the weight of the water the proportion of hydrogen in a unit volume of gas could then be calculated. The percentage volume of carbon dioxide contained in the gas being known, its weight was deducted from the weight of the total quantity obtained in the combustion. The difference is the quantity corresponding to carbon in the form of hydrocarbons. The nitrogen having been determined in a separate portion of gas, and the free hydrogen being also known, the volume of the hydrocarbons will be expressed by the following equation:

$$\left.\begin{array}{l}\text{C and H in form}\\ \text{of hydrocarbons}\end{array}\right\} = 100 - (CO^2 + N + H + \text{etc.})$$

That is to say, the actual volume of hydrocarbons will occupy the entire space in the gas not occupied by CO^2, N, and H, O and other constituents of the gas.

No attempt has been made to determine the proportion of individual members of the paraffin series—methane, ethane, propane, etc.—for the reason that no sufficiently accurate methods are known for the estimation of these bodies. No reagent can be named which will absorb and remove from a mixture any one of

these paraffins exclusively, so as to allow of its correct determination by difference.

In such a mixture, moreover, no decided chemical change can be produced in any given paraffin without more or less altering the others. They are remarkable for the resemblance existing between them in chemical relationships, and also for the great resistance which they offer towards reagents of every description, excepting chlorine, which attacks them all readily.

Moreover, a calculation of the relative proportions of the gaseous hydrocarbons of this class, based upon eudiometric data, is only possible where the number of such bodies is known to be limited to two—a condition never to be assumed in a gas of unknown composition.

In illustration of the fact just stated, it may be mentioned that a mixture of one volume each of methane, ethane and propane yields on complete combustion the same products and in the same proportions as three volumes of the intermediate hydrocarbon ethane. This can be shown by a very simple calculation.

SELECTION OF SAMPLES.

It was originally proposed to take samples from mains drawing gas from a group of wells, and in this way obtain an average of the entire group. This was sometimes done, as in the case of the Raccoon Creek and Speechley territories, where a large number of wells, all producing from one sand, are joined to one main. In other fields, the wells are often drilled to different sands, and produce gas from different horizons, as in the case of the Kane wells.

In many cases, among a large number of productive wells, all but two or three are shut in, and are thus held in reserve. In such instances a sample was taken at a single well, and directly from the main at the well.

Of the samples examined, No. 1 was taken at Fredonia, N. Y., by Mr. E. J. Crissey, Secretary of the Fredonia Natural Gas Light Company, from the mains of the company. All the other samples were collected by myself. In view of the great extent of the Pennsylvania gas territory, and the number of small areas of highly productive gas wells, the selection of samples with a view to an approximate average, is a matter of no small difficulty. For the present purpose, and in the absence of scientific criteria, reference

has been made chiefly to the technical importance of certain regions, such as Murrysville and Speechley. Fredonia, N. Y., was chosen, on account of the great depth (geologically) of the gas rock.

Wilcox gas is remarkable for the long maintained high pressure exhibited in certain wells.

Baden and Raccoon Creek lie on the same anticlinal.

Houston (Canonsburg) gas comes from a region 200 miles distant from the far northern Fredonia gas field. All the samples are from regions where natural gas is being largely utilized on account of its fuel value.

DESCRIPTION OF SAMPLES.

No. 1, Fredonia, N. Y. From mains of the Fredonia Natural Gas Light Company, May 12, 1887. Mr. E. J. Crissey, Secretary of this company, furnished the following information: Gas obtained at an average depth of 200 feet. The rock is black and gray shale, alternating, to the depth of about 1,000 feet, where a limestone is found. No gas has been found below 250 feet, until a depth of between 1,700 and 1,800 feet is reached, when gas and salt water are met. At 2,250 feet, gas is again found, which burns with a very white flame, whiter than that of the shallow gas. The sample examined comes from the depth of 200 feet.

Two determinations of nitrogen in this gas gave 9·58 and 9·50 per cent., respectively. Mean, 9·54 per cent.

In two determinations of carbon dioxide there were found 0·38 and 0·44 per cent. Mean, 0·41 per cent.

RESULTS OF ANALYSIS OF FREDONIA GAS.

Nitrogen,	9·54
Carbon dioxide,	0·41
Olefines,	0·
Carbon monoxide,	0·
Free hydrogen,	0·
Ammonia,	0·
Hydrocarbons of the paraffin series,	90·05
	100·00

343·47 cubic centimetres of Fredonia gas yield on combustion, by the method already described—

H^2O — 0·6254 gm.,	corresponding to H, — 0·06964 gm.	= 21·83	per cent.
CO^2 — 0·9144 gm.,	" to C, — 0·24938 gm.	= 78·17	"
		100·00	"

Making allowance for the 9·95 per cent. of nitrogen and carbon dioxide contained in the gas, it is calculated that the 90·05 per cent. paraffins present contain per litre—

0·80627 gm. carbon.
0·22515 gm. hydrogen.

In a second combustion of Fredonia gas, 326·17 cubic centimetres yielded—

H^2O — 0·5927 gm.,	corresponding to H,	— 0·0660	= 21·89 per cent.
CO^2 — 0·8635 gm.,	" to C,	— 0·23552	= 78·11 "
			100·00 "

As these quantities of carbon and hydrogen belong exclusively to the paraffins in the gas, it is calculated that the paraffins—amounting to 90·05 per cent. of the total gas—will contain per litre—

0·80185 gm. carbon.
0·2247 gm. hydrogen.

In these calculations, as in the following, an allowance is made in the determination of the carbon for the very small quantity of carbon dioxide, which always occurs in the original gas.

The means of the two results above cited are per litre of paraffins—

0·80406 gm. carbon,	= 78·14 per cent.
0·22492 gm. hydrogen,	= 21·86 "
	100·00 "

In the case of the Fredonia gas, no tests were made at the wells. An actual test made at one of the wells in August, 1884, showed traces of oxygen. In the limited quantity at disposal for the above analyses, no positively certain indication for oxygen could be obtained.

No. 2. From valve house, close to well No. 1, of the Sheffield Gas Company, one-half mile from Sheffield, Warren County, Pa. Wells Nos. 1, 2 and 3 were connected with the main at the time, so that the sample represents the average of the three wells.

Well No. 1 has been flowing since 1875; No. 2 was drilled two years later; No. 3 in 1885. The gas comes wholly from one and the same sand. The record of No. 1 is given on page 23, of Mr. Carll's Report on Warren County, for 1883.

The sand from which these wells produce gas is about 1,400 feet deep, and very nearly at ocean level.

The Sheffield Company owns six wells. In the newer wells, the pressure is even greater than in No. 1. The pressure in No. 1 has remained constant since it was drilled, and amounts to 550 pounds in four minutes, when the gas is shut in.

In the Sheffield region, there are about sixty-four square miles of gas-producing territory, and the gas pressure varies between 500 and 800 pounds per square inch.

The Sheffield gas wells supply Sheffield, Iona, Brookston, Clarendon, Warren, Corry, Erie and Jamestown, N. Y.

The wells in this region have been remarkably persistent.

Determinations of—	(1)	(2)	*Mean.*
Nitrogen,	9·00	9·12	9·06 per cent.
Carbon dioxide,	0·33	0·27	0·30 "

RESULTS OF ANALYSIS OF SHEFFIELD GAS.

Nitrogen,	9·06
Carbon dioxide,	0·30
Oxygen,	trace.
Hydrogen,	0·
Olefines,	0·
Carbon monoxide,	0·
Ammonia,	0·
Paraffins,	90·64
	100·00

305·27 cubic centimetres of Sheffield gas yield on combustion—

H^2O — 0·4960 gm.,	corresponding to H, — 0·05523 gm. =	23·36	per cent.
CO^2 — 0·6645 gm.,	" to C, — 0·18123 gm. =	76·64	"
		100·00	"

From these results, it is calculated that the paraffins present in Sheffield gas contain per litre—

0·65495 gm. caroon.
0·19960 gm. hydrogen.

In a second combustion, 314·44 cubic centimetres of Sheffield gas yield—

H^2O — 0·5090 gm.,	corresponding to H, — 0·05668 gm. =	23·27	per cent.
CO^2 — 0·6851 gm.,	" to C, — 0·18684 gm. =	76·73	"
		100·00	"

The paraffins will, therefore, contain per litre—

0·65557 gm. carbon.
0·19887 gm. hydrogen.

The means of these two analyses are per litre of paraffins—

0·65526 gm. carbon,	=	76·69 per cent.
0·19923 gm. hydrogen	=	23·31 "
		100·00 "

No. 3. Wilcox well, three miles from Wilcox, McKean County. Sample collected January 29, 1887. Originally known as "Wilcox Well No. 1," now called No. 7.

Was drilled in 1878, and produces gas from the fourth sand, exclusively. This well was the first in this region, and has maintained a continuous pressure of 500 pounds, when shut in. The United Natural Gas Company owns twenty-four wells in the Wilcox field, which occupies an area of about two miles square, No. 1 being in the southwest end. All are very productive, and some are remarkable for unusually high pressures, the gauge registering in one well 900 pounds. All exceed 500 pounds.

Very little salt water is produced. The gas exhibits a decided oxygen reaction, turns lime water rapidly milky, and has a strong odor.

Pipe lines carry the gas from these wells to Bradford, Jamestown, N. Y., Hornellsville, Salamanca and Buffalo, but the supply is largely in excess of the demand at present.

Determinations of—	(1)	(2)	*Mean.*
Nitrogen,	9·32	9·50	9·41 per cent.
Carbon dioxide,	0·21	0·20	0·21 "

RESULTS OF ANALYSIS OF WILCOX GAS.

Nitrogen,	9·41
Carbon dioxide,	0·21
Oxygen,	trace.
Carbon monoxide,	0·
Olefines,	0·
Ammonia,	0·
Hydrogen,	0·
Paraffins,	90·38
	100·00

374·2 cubic centimetres of Wilcox gas yield on combustion—

H^2O — 0·6022 gm.,	corresponding to H, — 0·06706 gm.	=	23·48 per cent.
CO^2 — 0·8014 gm.,	" to C, — 0·21856 gm.	=	76·52 "
			100·00 "

Hence one litre of the paraffins contains—

0·64622 gm. carbon.
0·19828 gm. hydrogen.

In the case of the Wilcox gas, an accident to some of the sample vessels prevented a second combustion, so that but a single result can be presented.

No. 4. Kane well No. 1, at Kane, McKean County. Gas collected January 30, 1887.

This well was drilled in 1884. The pressure then was 550 pounds when shut in for forty minutes. It was allowed to blow off for eight months and then shut in, when the pressure increased to 630 pounds. This gain in pressure has been permanent up to October, 1886, when the last test was made. The Kane Natural Gas Company owns two other wells in addition to this. The gas exhibits decided oxygen, and carbon dioxide reaction.

Determinations of—	(1)	(2)	*Mean.*	
Nitrogen,	9·67	9·91	9·79	per cent.
Carbon dioxide,	0·20	0·20	0·20	"

RESULTS OF ANALYSIS OF KANE GAS.

Nitrogen,	9·79
Carbon dioxide,	0·20
Oxygen,	trace.
Olefines,	0·
Carbon monoxide,	0·
Hydrogen,	0·
Ammonia,	0·
Paraffins,	90·01
	100·00

349·03 cubic centimetres of gas yield on combustion—

H^2O — 0·5600 gm., corresponding to H, — 0·06236 gm. = 23·18 per cent.
CO^2 — 0·7580 gm., " to C, — 0·20672 gm. = 76·82 "

100·00 "

Hence one litre of the paraffins contains—

0·65801 gm. carbon.
0·19849 gm. hydrogen.

248·1 cubic centimetres of the same gas yield on combustion—

H^2O — 0·3987 gm., corresponding to H, — 0·04439 gm. = 23·28 per cent.
CO^2 — 0·5366 gm., " C, — 0·14634 gm. = 76·72 "

100·00 "

Hence the paraffins of Kane gas contain per litre—

0·65537 gm. carbon.
0·19883 gm. hydrogen.

The means of these two analyses are per litre of paraffins—

0·65669 gm. carbon,	=	76·77	per cent.
0·19866 gm. hydrogen,	=	23·23	"
		100·00	"

No. 5. Speechley. This field has been a remarkably productive one, as regards quantity and pressure of gas and number of wells. These wells are situated on a northeast and southwest line, about six miles southeast from Oil City.

The sand rock from which the gas is obtained averages 1,900 feet in depth, and is about 900 feet below the third oil sand of Venango County. This latter sand also produces gas, but in much smaller quantity, and it is consequently, cased off, so that the gas in this territory is wholly obtained from one and the same sand rock. The Northwestern Gas Company, of Oil City, have sixty wells, and a large number of wells are owned by other companies.

The samples of gas for examination were taken April 13, 1887, from the high-pressure main at South Oil City, belonging to the Northwestern Natural Gas Company. At this time the pressure in the main was 100 pounds.

This sample may be considered to represent approximately the average of the gas from a large number of wells.

The tests at the main indicated the presence of oxygen, but less of carbon dioxide than found in the Warren and McKean County gas.

Determinations of—	(1)	(2)	*Mean.*	
Nitrogen,	4·61	4·41	4·51	per cent.
Carbon dioxide,	0·05	0·05	0·05	"
Hydrogen,	0·02	0·02	0·02	"

RESULTS OF ANALYSIS OF SPEECHLEY GAS.

Nitrogen,	4·51
Carbon dioxide,	0·05
Hydrogen,	0·02
Carbon monoxide,	0·
Olefines,	0·
Oxygen,	trace.
Ammonia,	0·
Paraffins,	95·42
	100·00

304·24 cubic centimetres of Speechley gas yield on combustion—

H^2O — 0·5423 gm.,	corresponding to H, — 0·06039 gm.	= 22·93 per cent.	
CO^2 — 0·7441 gm.,	" to C, — 0·20293 gm.	= 77·07 "	
		100·00 "	

Hence the paraffins of this gas contain per litre—

0·69900 gm. carbon.
0·20801 gm. hydrogen.

In a second combustion of the same gas, 310·52 cubic centimetres yield—

H^2O — 0·5500 gm.,	corresponding to H, — 0·06125 gm.	= 22·85 per cent.
CO^2 — 0·7585 gm.,	" to C, — 0·20686 gm.	= 77·15 "
		100·00 "

Hence the paraffins contain per litre—

0·69815 gm. carbon.
0·20671 gm. hydrogen.

The means of these two results are per litre of paraffins—

0·69857 gm. carbon,	= 77·11 per cent.
0·20736 gm. hydrogen,	= 22·89 "
	100·00 "

No. 6. Hukill well, on the Dick farm, Lyon's Run District, southern end of Murrysville field, and one of the sixty wells belonging to the Philadelphia Company.

This well was drilled in 1883, and was allowed to blow off for a long time. The well is very productive, and has a pressure as it flows through the main of 285 pounds.

The well has extra-heavy casing, and there is good reason to suppose that the gas comes exclusively from the Murrysville sand. The sample was taken April 8, 1887. The gas produces a decided carbon dioxide reaction, but exhibits a very slight reaction for oxygen.

This gas has a very faint odor, free from the pungent character noticed among some of the gas samples.

The well yields no oil, but a very little salt water.

Determinations of—	(1)	(2)	*Mean.*
Nitrogen, ...	2·13	1·91	2·02 per cent.
Carbon dioxide, . .	0·26	0·30	0·28 "

RESULTS OF ANALYSIS OF MURRYSVILLE GAS.

Nitrogen,	2·02
Carbon dioxide.	0·28
Oxygen,	trace.
Carbon monoxide,	0.
Olefines,	0·
Ammonia,	0·
Hydrogen,	0·
Paraffins,	97·70
	100·00

346·94 cubic centimetres of Murrysville gas yield on combustion—

H^2O — 0·5473 gm., corresponding to H, — 0·06095 gm. = 25·06 per cent.
CO^2 — 0·6682 gm., " to C, — 0·18224 gm. = 74·94 "

100·00 "

Hence the paraffins in Murrysville gas contain per litre—

0·53763 gm. carbon.
0·17981 gm. hydrogen.

In a second combustion, 306·28 cubic centimetres of gas yielded—

H^2O — 0·4818 gm., corresponding to H, — 0·05363 gm. = 25·02 per cent.
CO^2 — 0·5895 gm., " to C, — 0·16074 gm. = 74·98 "

100·00 "

Hence the paraffins contain per litre—

0·53718 gm. carbon.
0·17922 gm. hydrogen.

The means of the above analyses are per litre of paraffins—

0·53741 gm. carbon, = 74·96 per cent.
0·17950 gm. hydrogen, = 25·04 "

100·00 "

(*To be continued.*)

[*Contribution from the Department of Dynamics, University of Penna.*]

EXPERIMENTAL VERIFICATION OF WEISBACH'S THEORY OF IMPACT OF WATER UPON PLANE SURFACES.

BY W. C. SMITH, JR., and FRANKLIN SHEBLE.

INTRODUCTION.

The following series of experiments, made for the purpose of verifying laws of hydraulics, stated in *Weisbach's Mechanics of Engineering*, first volume, will prove of interest, as giving additional data in a direction which has received very little attention from experimenters, and also as proving the coefficient of efflux, usually adopted to be too small for the form of mouth-piece used.

WM. D. MARKS, Ph.B., C.E.,
Whitney Professor of Dynamical Engineering.

DESCRIPTION OF APPARATUS USED.

The Mouth-piece.—This was a circular orifice, of ½″ diameter, in a thin brass plate $\frac{1}{16}''$ thick by 3″ x 2½″. For such an orifice, Weisbach gives (p. 825) as a coefficient of efflux, ·626. We, by accurate experiment (described below), found this to be much too small. Under a head varying from 2·541′ to ·541′, the coefficient of efflux, with but one exception, remained unchanged to the

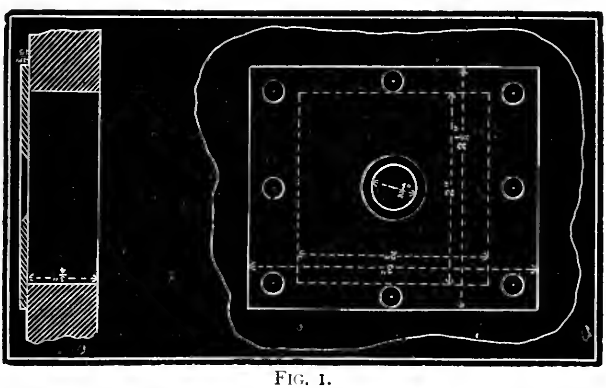

FIG. 1.

hundredths' place, and in the thousandths' changed from 1 to 2 and back again. This coefficient generally had the value ·712. The mouth-piece is shown in section in *Fig. 1.* The hole was bored with ½″ tool, and then reamed out with a larger one.

The Vessel used as a Water Tank.—This was a large barrel, giving as maximum head of water 2·541′. At the height of the different heads desired, above the orifice, overflow holes were bored, thus enabling the heads to be kept constant. The mouth-piece was placed upon the barrel, so as to be very approximately in a vertical plane. The size (2½″ diameter) of the orifice in the side of the barrel leading to the circular mouth-piece was so great compared with that (½″ diameter) of the actual orifice, as to exert no influence in over-contracting the effluent stream. The water was supplied to the barrel through a hose connected with the city mains.

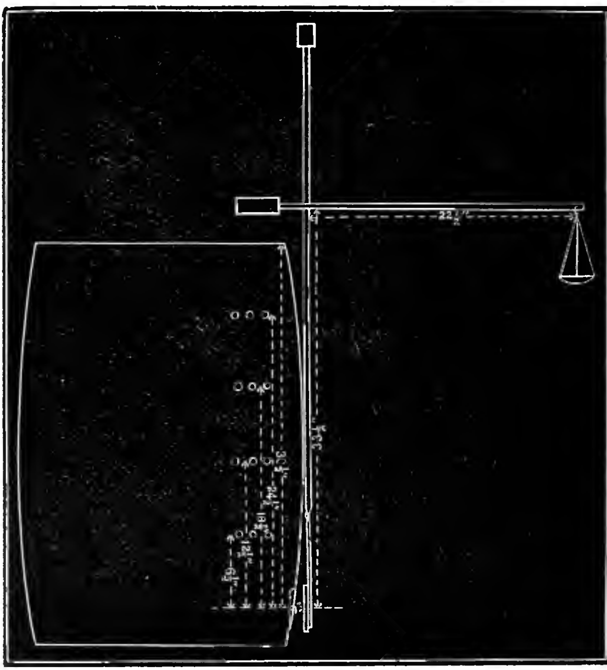

FIG. 2.

The Apparatus for Measuring the Strength of Impact.—This consisted of a bell-crank lever (*Fig. 2*), with arms 22½″ and 33½″ in length, and accurately counter-balanced. The arrangement for reducing the friction was not so good as might have been desired, for the lever, although supported somewhat above its centre of gravity, had a tendency to remain in any position in which it might be placed.

At the end of the longer arm, and fastened to it through the centre of pressure, was the surface impinged against by the effluent water. This was a board $4'' \times 4'' \times \frac{1}{4}''$, polished very smooth, so so that the force exerted on it should be that of the impact alone. (*Fig. 3.*)

In measuring the impact, the scale pan at the end of the $22\frac{1}{2}'$ arm was gradually loaded, until the surface struck the stream of water, and the weights were then increased, until the vertical arm came into coincidence with a plumb line hung from the supporting point of the lever. In this position, the mouth-piece

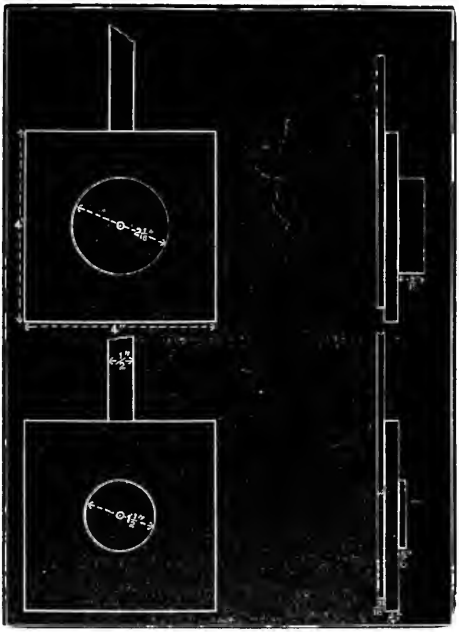

FIG. 3.

being vertical and very close ($1''$) to the surface, the axis of the stream was perpendicular to the surface as required. In bringing the surface to this position, it will be observed that the tendency of the friction of the lever was to increase the weight required in the scale pan. Accordingly, we see that the experimentally determined impact (vertical Column 4, Table III) is greater than that determined from the values of Q and c, corrected for the mouth-piece used (vertical Column 3, Table III), *i. e.*, greater than the theoretical values. This excess is also otherwise

accounted for in *Weisbach*, p. 1009, lines 21–24. It is probably due partly to each influence.

Consulting Columns 3 and 4, Table III, it is seen that the difference between the actual impact and the experimental impact is not constant, but decreases with the head. This shows that the friction of the machine, and the lengthening of the lever arm by the falling back of the water, and consequently their influence are decreased by shortening the head. It was for this reason, *i. e.*, influence of head, that we were unable to calculate and allow for the actual amount in ounces of the effect of friction, and of the length ening of the lever arm by the falling back of the water.

THE COEFFICIENT OF EFFLUX.

The method of finding the coefficient of efflux, μ, of the orifice was as follows: A cylinder of volume $V = \cdot 9303$ cubic foot was used to receive the water as it flowed unimpeded from the orifice. The time in seconds necessary to fill this vessel, with water flowing from the orifice with the velocity c_1, corresponding to the different heads (Column 1, Table I) is given in Column 2, Table I. The mean values of these observed times are given in Column 3, Table I. In Column 4, we have the actual amounts of water in cubic feet issuing from the orifice per second under the different heads. These are obviously V/t; t denoting the number of seconds required to fill the vessel V. In the fifth column we have the actual velocity with which the water leaves the orifice $= c_1 = Q_1 \div F$, Q_1 being obtained as in Column 4, and F representing the area in square feet of the orifice of ½″ diameter. For comparison, in the sixth column, are given the purely theoretical values in feet per second of the velocity of the effluent water, $c = \sqrt{2gh}$. In Table II, are tabulated the coefficients of efflux for the circular orifice ½″ diameter. In the first column are given the heads, in the second the purely theoretical quantity of water flowing out per second, calculated from the formula $Q = Fc = \sqrt{2gh}$, F and c having the same significance and values as in Columns 5 and 6. Table I, Column 3, is merely a reproduction of Column 4, Table I. In Column 4, Table II, are given the coefficients of efflux of the orifice for the different heads. The value of μ, for $h = 2{\cdot}041'$, is most unaccountably smaller than any of the others. These values of μ will be used (Column 3,

Table III) in calculating the actual impact against the surface. It is allowable to do this, for Weisbach, in deriving his theoretical formula $P = c\,Q\,\gamma/g = 2\,F\,h\,\gamma$, pre-supposes that the coefficient of efflux = 1, a case never realized in practice.

TABLE I.

For a Circular Orifice, ½ Inch in Diameter, in a Thin Plate.

V = ·9303 cubic foot. F = ·00136 square foot.

Head in Feet.	Time in Seconds.	Mean Time in Seconds $= t$.	Practical Q in Cubic Feet $= Q_1$ $Q_1 = \frac{V}{t}$	Real Velocity of Efflux in Feet $= c_1$ $c_1 = \frac{Q_1}{F}$	Theoretical Velocity of Efflux in Feet $= c = \sqrt{2gh}$
2·541	74½ 75 75	75·	·0124	9·117	12·79
2·041	85 84½ 85	85·	·0109	8·014	11·47
1·541	95 97 93 100 97	96·4	·0096	7·058	9·937
1·041	117½ 118 117	117·5	·0079	5·808	8·161
·541	163 164 160 164	163·	·0057	4·191	5·882

TABLE II.

For a Circular Orifice, ½ Inch in Diameter, in a Thin Plate.

F = ·00136 square foot.

Head in Feet.	Theoretical Q in Cubic Feet $= Q$. $Q = F\sqrt{2gh}$	Practical Q in Cubic Feet $= Q_1$. (From Table I.)	Coefficient of Efflux $= \mu$ $\mu = \frac{\text{Practical } Q}{\text{Theoretical } Q} = \frac{Q_1}{Q}$
2·541	·0174	·0124	·712
2·041	·0156	·0109	·699
1·541	·0135	·0096	·711
1·041	0111	·0079	712
·541	0080	·0057	·712

EXPLANATION OF THE TABLES.

Table I.—This has been already explained.

Table II.—This has been already explained.

Table III.—In this table, the first column contains, as usual, the different heads. In the second are given the purely theoretical impacts calculated from the formula in *Weisbach*, p. 1008. These are merely given for comparison, having no meaning other than that expressing the depressing influence of the orifice on the quantity Q. In Column 3 are given the impacts in ounces, calculated with reference to the actual values Q_1 and c_1 of Q and c. In the fourth column are given the impacts indicated by the lever and scale pan, and these are larger than those in the preceding column, as explained above.

Table IV.—This contains the result of experiments with a bounded surface. The first column gives the heads in feet. The second contains merely the values of Column 2, Table III, multiplied by 2, and the third those of Column 3, Table III, multiplied by 2. The fourth column is Column 4, Table III, multiplied by 2, and the fifth and sixth columns contain the experimental values of the impact against two bounded surfaces of different dimensions.

Table V.—Explains itself.

TABLE III.

For a Circular Orifice, ½ Inch in Diameter, in a Thin Plate.

Impact against a Plane Surface.

$Q_1 = \mu\, Q.$

Head in Feet.	Theoretical Impact in Ounces. $J = 2\,F\,h\,\gamma = \frac{c}{g}\,Q\,\gamma$	Actual Impact in Ounces, Using c_1 and Q_1, from Table I. $J_1 = \frac{c_1}{g}\,\mu\,Q\,\gamma = \frac{c_1}{g}\,Q_1\,\gamma$	Experimental Impact in Ounces. $= I_2$.
2·541	6·91	3·51	3·94
2·041	5·55	2·71	3·18
1·541	4·19	2·10	2·35
1·041	2·83	1·42	1·59
·541	1·47	·74	·92

TABLE IV.

For a Circular Orifice, ½ Inch in Diameter, in a Thin Plate.
Impact against a Bounded Surface.

Head in Feet.	Theoretical Impact in Ounces. $2I = I_h = 2Fh\gamma = \frac{2c}{g}Q\gamma$.	Actual Impact in Ounces $= 2I_t = I_h = \frac{2c_1}{g}\mu Q\gamma = \frac{2c_1}{g}Q_1\gamma$	Experimental Impact in Ounces. (From Table III.) $I_{2h} = 2I_e$.	Experimental Impact in Ounces against Surface 2 1-16 inches Diameter and 9-16 inch Deep.	Experimental Impact in Ounces against Surface 1½ inches Diameter and ⅜ inch Deep. Being one-half Size of that in Art. 501.
2·541	13·82	7·02	7·88	7·81	7·64
2·041	11·10	5·42	6·36	6·30	6·13
1·541	8·38	4·20	4·70	4·62	4·53
1·041	5·66	2·84	3·18	3·11	3·11
·541	2·94	1·48	1·84	1·76	1·76

TABLE V.

For a Circular Orifice, ½ Inch in Diameter, in a Thin Plate.
Showing Relation between Experimental Impacts against Plane and Bounded Surfaces.

Head in Feet.	Experimental Impact against a Plane Surface.	Experimental Impact against a Bounded Surface, 2 1-16 inches Diameter and 9-16 inch Deep.	Experimental Impact against a Bounded Surface, 1½ inches Diameter and ⅜ inch Deep. (One-half Size of that used in Art. 501.)
2·541	1·	1·98	1·94
2·041	1·	1·98	1·93
1·541	1·	1·97	1·93
1·041	1·	1·96	1·96
·541	1·	1·91	1·91
		mean, 1·96	mean, 1·934

These tables, whose accuracy we can vouch for, seem to show beyond all doubt, that Weisbach's theory of impact, if the influence of the mouth-piece be allowed for, is practically true as well as theoretically.

[*Contribution from the Dept. of Civil Engineering, University of Penna.*]

IMPROVEMENT OF TIDAL RIVERS.

BY LEWIS M. HAUPT, A.M., C.E., Professor of Civil Engineering.

By the improvement of rivers is meant the removal of any or all obstructions to navigation, as bars, reefs, ledges, snags, bowlders, bends, wrecks, etc. It consists, in fact, in the rectifying and deepening of the channel to such an extent as to adapt it to meet the requirements of commerce. The engineer must therefore ascertain what these requirements are, as well as the character and cause of the obstructions, before he can apply the remedy. This involves a thorough survey and study of the physical features of the stream, the determination of its velocities, measurement of its volume, and sections and location of its thalwegs, that the conditions of its discharge may be known.

Since, in alluvial bottoms, bars form the chief obstructions, an investigation into their cause becomes of fundamental importance. As a basis for this investigation, it may be accepted as a maxim that *if the bottom velocity of a stream be increased to the limit required by the material, it will scour; if diminished, it will deposit*, hence whatever tends to increase its energy at any point, will prove beneficial at that point, and whatever tends to diminish it will be injurious. The energy being a function of the mass into the square of the velocity, and the mass being constant, it follows that the bar-building or bar-destroying agencies vary as the squares of the velocities, hence the importance of maintaining a uniform velocity sufficient to prevent the formations, or secure the removal of bars.

But the tendencies of flowing water are in the opposite direction, or towards impulsive movements. In moving down a uniform slope the velocity is augmented until it may become sufficient to move the material composing the bed. This is then pushed or rolled along, until a low dam is built up, which checks the velocity and forms a pool over the crest of which the water escapes, only to repeat the operation. Thus the profile of the bed of a stream will be found to be a succession of hills and hollows, with the more gentle slopes on that side of the crests from whence came the

material. The velocity is also affected by the volume of water passing a given section in a given time, or the prism of discharge; by the area of the section; by the form and material of the channel, and by other conditions, all of which must be considered in any attempt to modify the action of the currents at any point.

BARS.

In tidal waters the problem becomes more complicated, in consequence of the opposing forces of the flood and ebb tides flowing often in different channels and with constantly varying velocities. In straight reaches of the river bed, the paths of flow will be coincident, but in reverse curves it is evident that the momentum of the flood will tend to impel it against one bank, whilst that of the ebb will force it to the opposite side, thus separating the paths of the resultants and leaving between them a line of lower velocity where a bar will ultimately form. This may be called a *cross-over bar*. Again, where the water passes from a contracted or narrow section of its bed to a wider section without a corresponding increase of volume, there will be a dispersion of its energy and reduction of velocity, causing a precipitation which may be designated as an *expansion bar*.

A *confluent bar* is one invariably found at the junction of two streams, where the velocity of one is checked by the inertia of the other. These bars will vary in size and position according as the angle between the axes of the streams vary. They are also affected by the relative volumes of the streams.

A fourth cause for the formation of bars is found in the loss of velocity, due to a change of direction. At every bend, below the convex bank, there will be found more or less of a deposit. In tidal rivers, it is often visible, both above and below the turn. These formations may be termed *elbow bars*.

Such are the principal causes of bars, and they at once suggest the correspondent remedies. Thus, to remove *cross-over bars*, both tides should be made, if possible, to flow in the same path, or the volume of either prism should be modified by regulating works, properly located. *Expansion bars* may be prevented or removed by contractions by lateral dikes or by canalization. *Confluent bars*, by contractions over limited areas, by closing lateral channels, usually at the upper end, or by a change in the direction of the

axis of either stream, where possible. *Elbow bars* are not serious obstructions, but where necessary, they may be partially removed by wing dams.

Dredging cannot be expected to produce permanent results, except where there are auxiliary works to change the regimen of the stream and increase the bottom velocities over the area dredged. It follows from the above, that jetties or bulkheads, which are so located as to change the direction of a stream *abruptly*, whether in a river or harbor, will produce elbow-bars and so prove injurious when placed in or near the channel to be created. Such jetties are usually built in pairs to defend the artificial channel dredged between them, and at an angle to the path of the current, in which case they form slackwater pools for the accumulation of silt, which requires to be frequently removed. Instead of increasing the velocity of the stream, they act as obstructions to the currents and tend to diminish the velocities both above and below them, forming bars.

It has been frequently noted by the most casual observer, that the deep-water channel of a river is to be found near to the concave bank, at the bends. This is due to the "head" produced by the momentum, which piles the water up on that side, and also to the increased velocity of that portion of the stream, since it traverses the longer path. This effect is, therefore, produced by the form of the bank, which *reacts* on the stream, throwing it back upon itself and changing its direction, and the same results may invariably be produced by similar artificial forms of construction, which I would designate as *reaction dikes*, placed adjacent to the bars to be removed, to distinguish them from deflecting dikes, located at a distance from the site to be improved.

Before proceeding to a consideration of any particular river as a whole, it should be further noted that, as a rule, the *flood* tide is the bar-building and the ebb the bar-destroying agent. For, as the flood rolls up, it must retard, stop and reverse the movement of the ebb augmented by the land drainage; its waters are heavier than those of the ebb under which it flows as a wedge, lifting them to the surface, so that whilst the bottom currents are running *up* stream on the surface, the ebb continues flowing *down* for nearly an hour after the lowest water has been observed. This is shown by the rapid rising of the water on the gauge—for

a foot or over—in mid-stream, before the surface currents are reversed.

It is this submerged flood action wedging its way up stream, resisted and forced under by the weight of the superincumbent ebb, which displaces the sand and silt of the bed of the river, and where the resultant of the ebb discharge does not effect its outflow over the same path, there the deposit is left and a cross-over bar formed.

Again, wherever a salient or projecting bank occurs, flanked by reverse curves, it will be found that the loci of the flood and ebb resultants will be separated, the former hugging the convex bank, against which it impinges, on the down-stream side, and under which it scours a channel, gradually diminishing in depth to a point just below the elbow; thence it crosses to the concave bank above the bend, with a steeper slope, leaving below it two well-defined channels, with a submerged spur, or often an island, between them. The one on the convex is the flood, whilst that on the concave bank is the ebb channel. When these can be made to coincide, deeper water will be the result.

PHYSICAL CHARACTERISTICS OF THE DELAWARE RIVER.

The application of these principles becomes manifest at once by reference to the accompanying plan and profile of the Delaware River. By an inspection of the plan, it will be seen how readily the location of the bars may be determined. Passing up stream from the head of the bay below Reedy Island, we find a long straight reach succeeded by a double flexure, causing the paths of the flood and ebb to diverge and forming the cross-over bar at Reedy Island.

The broken line on the chart indicates the location of the thalweg or line of deepest water, which is sometimes in the ebb channel and at others in the flood. The figures indicate the depths in feet below mean low water, as determined by the United States Coast Survey of 1840–44, and subsequently.

In approaching Reedy Island, the direction of the flood resultant is such as to intersect the shore just below Port Penn (see chart), where it is compressed into the narrow channel back of the island, producing a depth of thirty feet. Thence it proceeds close along shore with decreasing depth to Reedy Point, where there are but

six feet just below the point; then a steep descent to twenty-five feet in the ebb channel beyond. Between the flood channel on the left and the ebb flowing under Elsingborough Point lies the bar of which Reedy Island forms a part. On the opposite or convex bank of the river and protected from both flood and ebb by Stony and Elsingborough Points, are seen the elbow bars, known as Stony Point and Dan Baker Shoals. This latter being near mid-river, is a dangerous obstruction, and works are now being executed for its removal, at an estimated cost of $324,000. The original plan to build a concave dike, about 25,000 feet long, or nearly five miles, from Reedy Island to a point on the right bank below Blackbird Creek, has been modified so as to leave the lower end open to provide for navigation. Such a dike will, no doubt, reduce the flood velocities behind the island and thus increase the elbow bar at Reedy Point. This will in turn deflect somewhat more of the ebb, causing it to act more directly upon the shoal. By this indirect application and at a large expense, it is hoped that a sufficient scour will be effected. The difficulties of securing a foundation for the dike in the semi-fluid mud at this locality are very great, and it is reported that solid bottom lies at a depth of ninety feet below the surface. About 2,000 feet of this work is under construction. It is my impression that by a more direct application of the ebb, the resultant of which lies to the east of the axis of the river, by means of a reaction dike, located between the Dan Baker Shoal and Alloway's Point, which would be only about one-third as long and cost correspondingly less, a better result would be obtained.

In the next reach of the river, from Reedy Point to New Castle, the same features occur in reverse order. Here, the flood is the eastern and the ebb the western channel; the elbow bars are found at Goose Island Flats, and the cross-over bar in the Pea Patch Island and Bulkhead Shoals.* Here, also, is seen an *expansion* bar, known as the Middle Ground, due to the abnormal width of the river at this bend. The dikes proposed by the Board of Engineers for the improvement of the east channel are represented by the dotted lines on the chart. The one, extending from the island

* It was here that in 1873 the writer established the lines and surveyed the sites for the first range lights located and built upon the Delaware, under the direction of General Wm. F. Raynolds, United States Light-House Engineer.

"up stream in a curved line approximately parallel to the New Jersey shore opposite—14,000 feet, or less, in length," it is estimated, will cost about $231,000. The cost of dredging at this locality, to give a channel 26 by 600 feet, is estimated at $88,020.*

From New Castle up, the river is nearly straight for about thirteen miles, but it varies in width through a range of about 100 per cent. At the widest part is seen the Cherry Island Flats, which is an expansion bar. The crest at the lower end of the western channel has been dredged to a depth of 24 feet, width of 400 feet, and length of 2 miles, between the years 1880 and 1884, at a cost of nearly $400,000, and is more nearly self-sustaining than any other work of a similar character on the river, as it lies in the path of both flood and ebb currents. The tendency of the river to build a bar on the north side would indicate that the southern channel were the better one to improve with a view to permanence. The Marcus Hook Bar, at the upper end of the straight reach, is virtually an elbow bar formed by the flood above Old Man's Point, and is out of the way of navigation. Just beyond this, however, occurs the rocky shoal known as Schooner Ledge. As this is rock, its presence is not due to the action of the river; its removal by blasting has already been in large part completed, and it is permanent. The estimated cost of its total removal to twenty-six feet depth is about $450,000.

Cherry Island is an elbow bar, produced by Thompson's Point, whilst the middle grounds of Little Tinicum and Maiden Islands with the Fort Mifflin Bar are combinations of cross-over and elbow bars, producing one of the most difficult sections of the river to improve. On the bar separating the ebb and flood channels, there was originally but seventeen feet of water. "Dredging was begun in 1873, and has continued at intervals ever since, but the river has not been able to maintain the depth so gained." The cut was nearly a mile long and 400 feet wide and had cost, up to 1885, $281,784. During 1885, 96,145 cubic yards more were removed and deposited behind the brush and stone dike, then under construction between Hog and Maiden Islands. "The filling seems to have been the result * * * partly of the washing in of sand by the flood current which passes up the channel behind

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* From the *Preliminary Report of the Board of Engineers, U. S. A.*, dated Philadelphia, Pa., January 23, 1885.

Tinicum Island and crosses the line of the cut diagonally." It is to cut off this drift that the present dike, 7,000 feet long, is being built to mean low water.

At the entrance to the Schuylkill there is found the usual confluent bar, which has been frequently dredged and as frequently filled up. Above this, within the limits of Philadelphia, are the horse-shoe, chiefly an elbow bar; Smith's and Windmill Islands, cross-over; Richmond or Petty's Island, an expansion bar, and Five Mile, a cross-over and elbow bar. This last-named obstruction is so marked in growth and so serious a barrier that it forms the upper limit to deep draught vessels, and is the controlling feature in all up-river navigation. But for this bar there could readily be maintained a sixteen foot navigation almost to Trenton, or at least to Bordentown. Its partial removal therefore becomes an important matter in the permanent improvement of the river, and it is my purpose to give it detailed study, the results of which I hope to submit hereafter.

The remaining up-river bars, with their characteristics and possible remedies, may be readily traced from the plans and profiles of the river. In these thirty miles, it will be observed how the intermittent movement of the stream moulds the bottom thalweg into gentle up- and steeper down-river slopes. The Five Mile Bar is an exception, and indicates that the movement is in the opposite direction.

SUMMARY OF IMPROVEMENTS BELOW PHILADELPHIA.*

The original obstructions at Mifflin Bar reduced the depth to seventeen feet; at Schooner Ledge and Cherry Island Flats, to eighteen; at Bulkhead and Dan Baker Shoals, to about twenty feet, M.L.W.

"The efforts of the past to improve the river between Philadelphia and the bay have been confined to dredging, except at Schooner Ledge, where solid rock has been removed."

"The entire amount expended on the improvement of the Delaware River, from 1836 to June 30, 1885, was $1,364,746.87, of which $45,000 was expended on that part of the river between Trenton and the upper part of Philadelphia."

"During the fiscal year ending June 30, 1886, the sum of

* Extract from *Report of the Chief Engineer, for 1886.*

$134,971.05 was expended in surveying," dredging and dike construction.

Of the various channels dredged, some were entirely obliterated within a year, and others are slowly but surely filling. The only permanent work has been the partial removal of Schooner Ledge to twenty-four feet, M.L.W. Hence in fifty years there have been expended in dredging, surveying and diking (deducting the $170,000 spent on Schooner Ledge and $40,000 for up-river), $1,284,717.92, and in January, 1885, a board of engineers, appointed to prepare plans and make an estimate for the permanent improvement of the river, reported that the formation of a ship channel, having a least width of 600 feet and a depth of 26 feet, M.L.W, "by means of dikes, with recourse to dredging where necessary, as an aid to such contracting and regulating works," would cost about $2,425,123 for the lower portion of the river. The cost of the maintenance of these works is estimated at $87,206, which is but 3·6 per cent., and the interest of the capital at four per cent., is $97,005.92, making the total annual expense $184,211.92, as compared with $268,439, the estimated annual cost of dredging. No limit of time can be fixed within which the results may be secured. The report also adds that "regulating works, if their positions are properly marked, are an aid to navigation."

As to the efficiency of the two methods, by dredging or by diking, there can therefore be no question unless the cost of dredging can be reduced to a small percentage of that used as a basis in the estimate, and this, I believe, can be done at some localities by utilizing the surface velocities with current deflectors, which are found to be very rapid and effective. But even without such a current-dredge, the magnitude of the interests involved is so great as to require the critical study of each site with a view to the selection of that form, size and position of dike which will give the best results with the least expense for cost and maintenance. Such an effect, I believe, can be secured by a *reaction dike* built in the form of a reverse curve or ogee, properly located with respect to the currents, and so placed as to compress and concentrate the ebb locally upon and carry it over the obstructing bar.

The use of dikes, as generally applied, is but a feeble, indirect and expensive way of applying the principle of current

scour. It acts over large areas, and is very slow in its operation, sometimes requiring scores of years, whereas the local construction should be more efficient and immediate for the reasons stated.

WAS PHILIP REIS THE INVENTOR OF THE ARTICULATING TELEPHONE?

BY PROF. EDWIN J. HOUSTON.

There appeared in the January number of the JOURNAL OF THE FRANKLIN INSTITUTE, for 1887, a short article by me, entitled "Can the Original Reis Telephone transmit Intelligible Articulate Speech?" In this article was published some correspondence from Dr. Theodore Stein, of Frankfort-on-the-Main, in reference to certain experiments made by Mr. Jno. R. Paddock with old and original Reis instruments. In this correspondence, Dr. Stein denies that the apparatus in question can transmit articulate speech, but acknowledges that certain other forms of Reis apparatus can transmit intelligible words or short sentences.

Dr. Stein has published in the *Electrician and Electrical Engineer*, for July, 1887, an open letter to me, which I have only just seen, since its author omitted sending me a marked copy of the same, and since he preferred to publish his letter in a different journal from that in which my article appeared. It is true that I am a subscriber to the excellent journal in which Dr. Stein's open letter appeared, yet, from great press of professional work, my journals had accumulated during the summer, unread, in a manner which I am sure not infrequently happens to busy men. I have therefore only just seen Dr. Stein's letter, and if it has remained unanswered so long, he has only himself to blame therefor.

Since Dr. Stein has chosen the form of an open letter, I may perhaps be pardoned for replying in this somewhat questionable style of conducting a scientific controversy.

PHILADELPHIA,
To Dr. Theodore Stein: September 14, 1887.

It is only at this late date that your open letter to me of May 25, 1887, has come to my notice. Had you seen fit to publish the same in the JOURNAL OF THE FRANKLIN INSTITUTE, in which the statements to which you object

LEWIS
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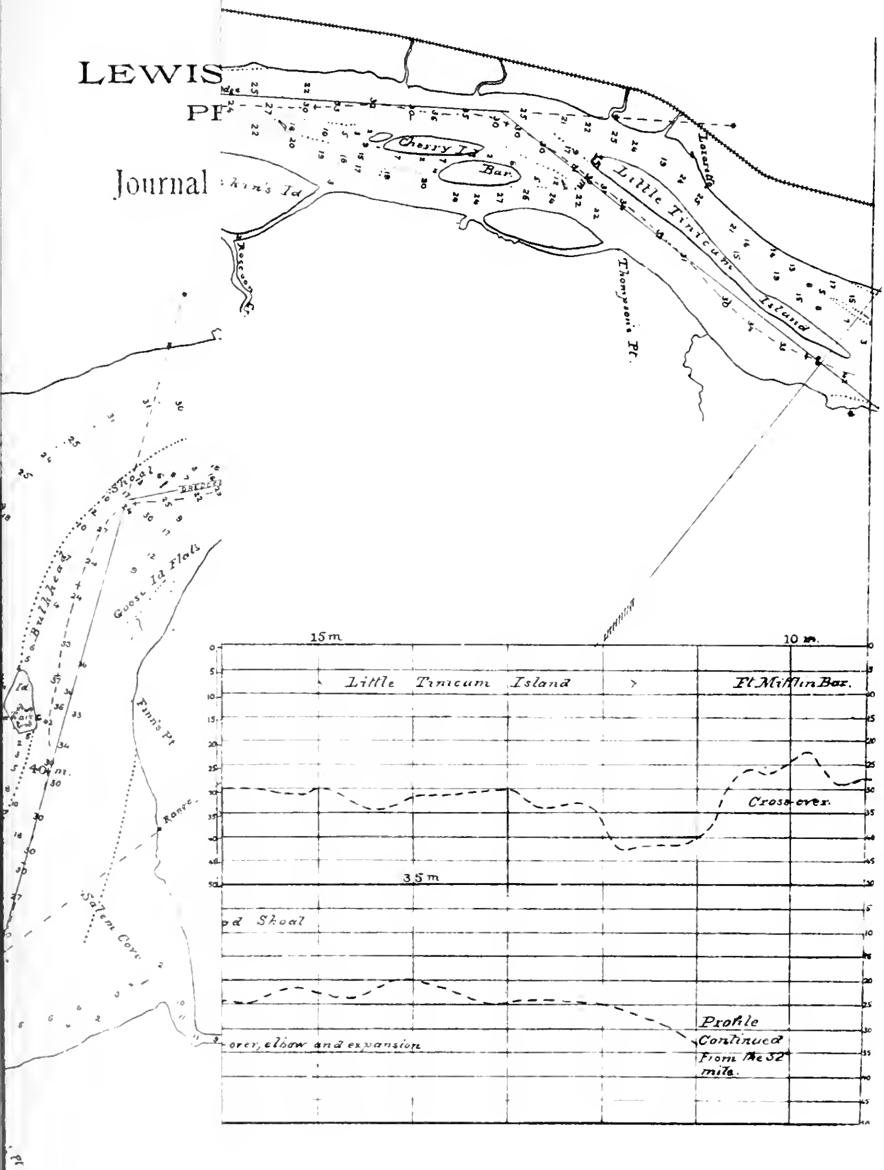
Journal



Jour. Frank. Inst., Vol. CXXIV. October, 1887.

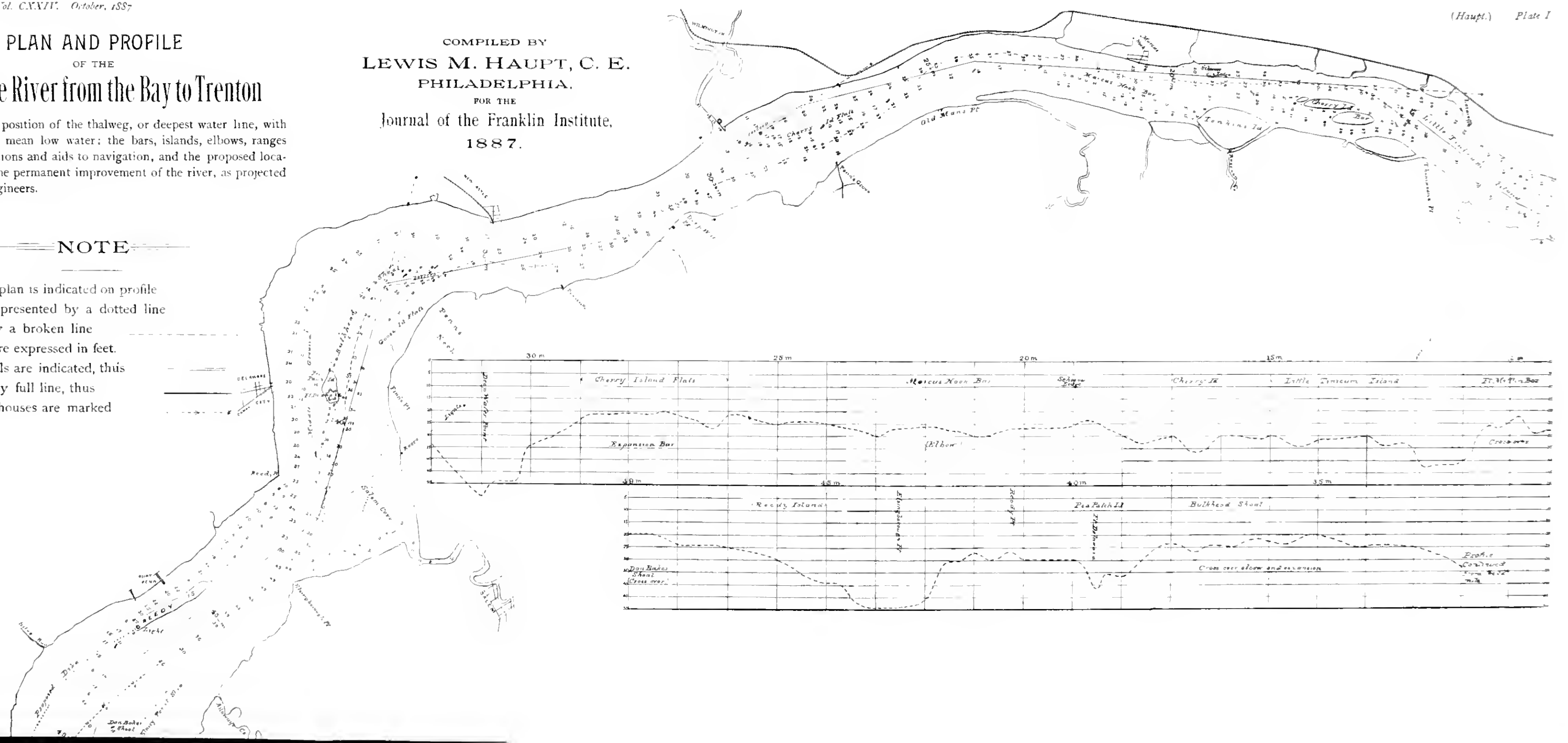
PLAN AND PROFILE
OF THE
Delaware River from the Bay to Trenton

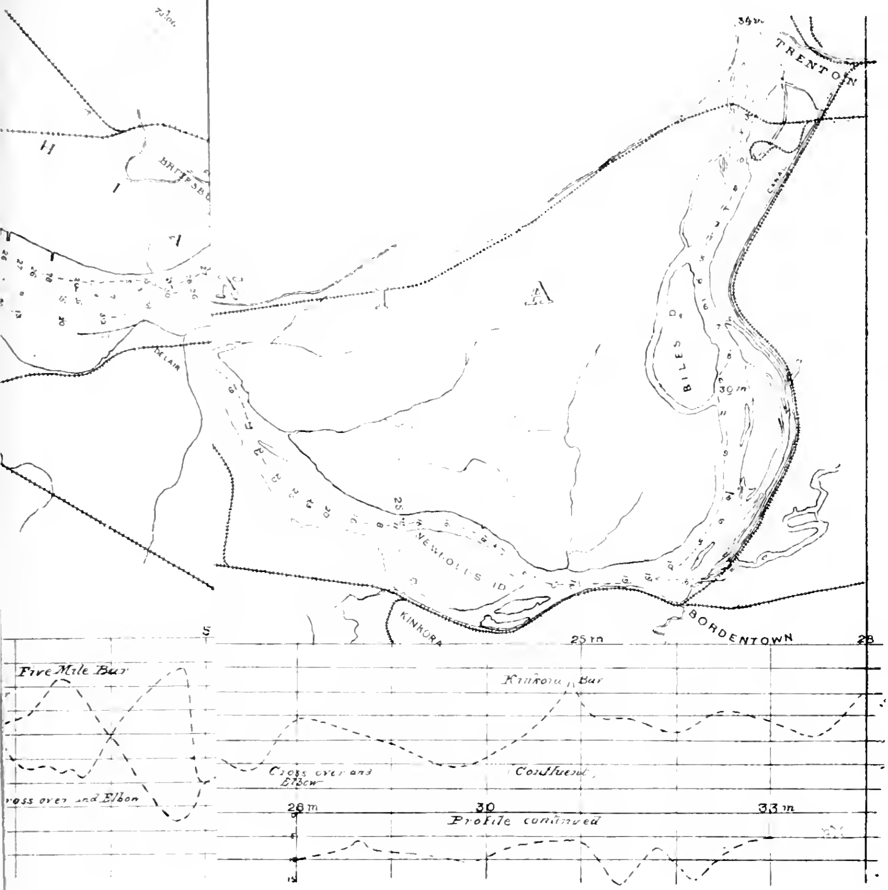
SHOWING the position of the thalweg, or deepest water line, with depths below mean low water; the bars, islands, elbows, ranges and other obstructions and aids to navigation, and the proposed location of dikes for the permanent improvement of the river, as projected by a Board of Engineers.

COMPILED BY
LEWIS M. HAUPT, C. E.
PHILADELPHIA,
FOR THE
Journal of the Franklin Institute,
1887.

NOTE

The scale of the plan is indicated on profile
The dikes are represented by a dotted line
The thalwegs by a broken line
The soundings are expressed in feet.
Dredged Channels are indicated, thus
Dikes by a heavy full line, thus
The range light-houses are marked





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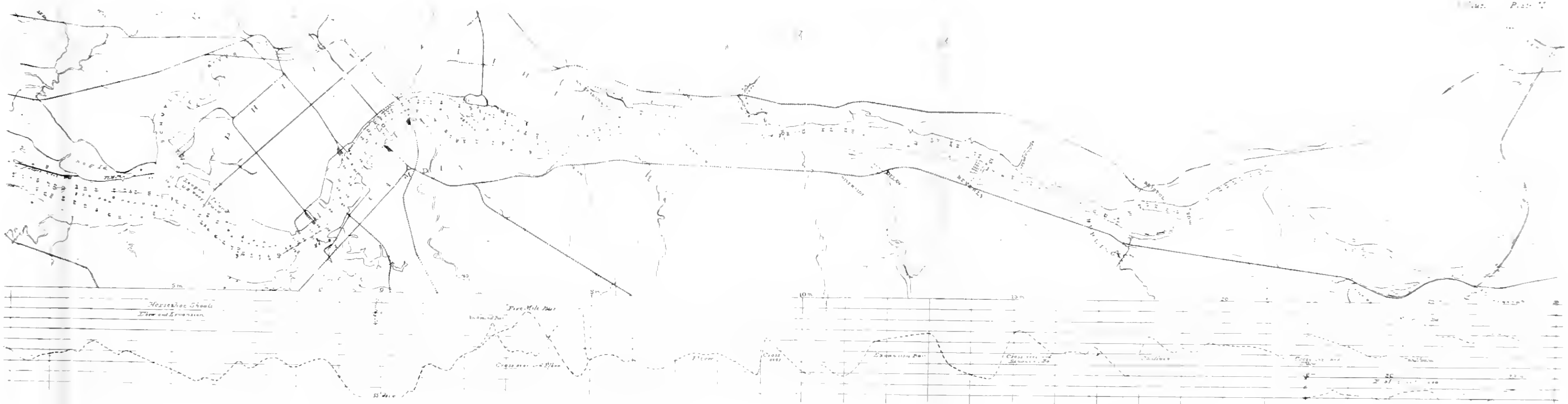

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appeared, or had you sent me a marked copy of the paper in which you did publish, you would not have been kept waiting so long for my reply.

The burden of your objection appears to rest on my understanding of your letter of October 23d, in which certain remarks are made concerning the platinum contact and its opposing contact piece on the telephone membrane.

I quite agree with you that "such a nonsensical arrangement is against all common sense," and, indeed, as you will probably recall, I stated in my article in the January number of the JOURNAL: "It is difficult for us to believe that Dr. Stein could have so blundered as to experiment with an open-circuited apparatus." Nevertheless, your letter of the 23d of October is open to this interpretation, and I do not understand that in your open letter to me of May 25th, you deny the correctness of the translation of said October letter, since you say in your open letter: "The sentence in my letter to you of October 23d, was intended to mean that through the vibration of the membrane, while speaking or singing in the wood cube, the *platinum point* was lifted from the membrane and thus a transmission of sound through an interrupted circuit was made." You will, of course, recognize the fact that you can hardly hold me responsible for what you *intended* to say, but only for what you actually said.

I am quite willing, however, to accept your explanation as to what you intended to say with respect to the platinum contact and its position in relation to the telephone membrane, but would call your attention here to the unfortunate antithesis you made between this form of Reis instrument, and that other form of Reis instrument in which the platinum contact "rests directly on the membrane by gravity." If your meaning is that in the apparatus in controversy, the platinum contact *when not in use* rests on the membrane, but *when in use is thrown off from the membrane*, thus producing the breaks or interruptions in the circuit, which you allege prevent the transmission of articulate speech, then I am presumably correct in assuming that in the later form of Reis apparatus the platinum contact *not only rests on the membrane when not in use, but also when in use*, otherwise there would be no significance in your contrast of the two pieces of apparatus; or, as you say in you letter of October 23d: "By reason of this, there are here no interruptions of the current, but only fluctuations (undulations) in it, which is the desideratum for the production of articulate words or sentences." If you have been correctly translated here, you may perhaps understand my surprise at your failure to appreciate that just here lies the quintessence of the entire controversy. If Philip Reis did this, then he was the inventor of the articulating telephone, since he devised an apparatus that enabled articulate speech to throw electrical undulations on a circuit, which I suppose you will admit can transmit articulate speech.

It would appear, then, from your own testimony, that Reis was the true inventor and Bell merely the improver. The question is not, as you appear to think, who invented "the first useful apparatus for the practical far-speaking telephone," but who invented the articulating telephone? No one can deny that, taken as a whole, Mr. Bell's apparatus is better suited for

practical work than that of Mr. Reis, but Mr. Bell's apparatus constitutes, in the opinion of many well-known and reputable physicists in this country, a mere improvement on an apparatus already invented, and was not a new discovery or invention.

Since your open letter tacitly implies a wilful misunderstanding on my part of your letter of October 23d, I will here reproduce the original letter and the translation of the same, and leave the verdict concerning the matter to such of the scientific world as may be interested therein:

ELEKTROTECHNISCHE RUNDSCHAU,
Redaktion Kaiser Strasse 25, Frankfurt a. M.

FRANKFURT, A. M., den 23. Oktober, 1886.

HERRN PROFESSOR EDWIN J. HOUSTON, IN PHILADELPHIA.

SEHR GEEHRTER HERR!—Ich habe durchaus nichts dagegen einzuwenden, wenn Sie den Ihnen am 21. Juli d. J. geschriebenen Brief veröffentlichen.

Trotz Ihrer gefälligen Mittheilung, dass Sie selbst keinen Zweifel in die Angaben des Herrn Professor Paddock setzen, muss ich bei meiner Behauptung stehen bleiben, dass man mit dem Apparate, welchen ich Herrn Professor Thompson überlassen habe und den Herr Professor Thompson an die Overland Telephone Company weiter gegeben hat, nicht, wie mit den neueren Bell-Telephonen, Worte oder ganze Sätze, auf irgend eine Entfernung, auf die man nicht durch direkte Sprachvermittelung hören kann, zu übertragen vermag. Solches ist nur mit den Reis'schen Telephonen zweiter Form möglich (abgebildet in Thompson's Buch: *Philip Reis, Inventor of the Telephone*, Seite 86), weil man an diesen Apparaten die Membran beliebig spannen kann, und weil hier der Platinkontakt nicht, wie bei meinem, Herrn Professor Thompson überlassenen Apparate, etwas von der Membran absteht, sondern direkt nach dem Gesetze der Schwere auf der Membran ruht, mithin hier keine Stromunterbrechungen stattfinden, sondern nur Stromschwankungen, wie solches das Desiderium bei Hervorbringen von artikulirten Worten oder Sätzen ist.

Die kürzlich publicirte Beobachtung von Francis E. Nipher, dass man bei einer bestimmten Spannung der Membran mittelst des Reis'schen Telephons Worte übertragen könne, halte ich für vollkommen richtig und bin überzeugt, dass jedes Mal, wenn, wie vor 25 Jahren, Worte oder kleine Sätze mit dem Reis'schen Telephon zweiter Ordnung übertragen haben, die Membran richtig gespannt war; es war immer ein Zufall, wenn dies geschah, da Reis selbst in den Vorträgen über sein Telephon auf die richtige Spannung der Membran kein besonderes Gewicht gelegt hatte.

Wenn Sie auch diese Mittheilungen im JOURNAL DES FRANKLIN INSTITUTS publiciren wollen, habe ich nichts dagegen einzuwenden.

Ich bin mit vorzüglicher Hochachtung,

Ihr ergebenster DR. STEIN.

[*Translation.*]

ELEKTROTECHNISCHE RUNDSCHAU,
Editoral Office: Kaiser Strasse 25, Frankfurt a. M.

FRANKFURT A. M., October 23 1886.

TO PROFESSOR EDWIN J. HOUSTON, IN PHILADELPHIA.

DEAR SIR:—I have not the slightest objection to the publication of my letter to you, dated July 21, 1886.

Notwithstanding your statement, that you have no reason to doubt the accuracy of the declaration of Professor Paddock, I must stand by my assertion, that, with the apparatus which I gave to Professor Thompson, and which he subsequently gave to the Overland Telephone Company, it is not possible, as it is with the more recent Bell Telephone, to transmit words or whole sentences to any distance beyond that at which the spoken words may be heard directly. Such transmission is only possible with the Reis telephone of the second form (illustrated in Thompson's book: *Philip Reis, Inventor of the Telephone*, page 86,) for the reason that in this apparatus the membrane may be tightened to any extent, and also because in this, the platinum contact does not stand-off somewhat from the membrane, as in the apparatus which I gave to Professor Thompson, but rests directly upon the membrane by gravity. By reason of this, there are here no interruptions of the current, but only fluctuations (undulations) in it, which is the desideratum for the production of articulate words or sentences.

I consider the recently-published observation of Francis E. Nipher, that, by giving the membrane a certain tension, it is possible to transmit words with Reis's telephone, to be quite correct; and I am convinced that every time we succeeded, 25 years or more ago, in transmitting words or short sentences, with the second form of the Reis telephone, the membrane had just the right tension. It was always an accident, when this happened, for Reis himself, when lecturing on his telephone, attached no particular importance to the proper tension of the membrane.

Should you wish also to publish this communication, you are at liberty to do so. I am with high esteem, etc., DR. STEIN.

I note what you say as to the bored-block transmitter, etc., "Nevertheless, neither Philipp Reis himself nor I, *nor any other German experimenter*, (my italics) has ever been able to converse with a Reis bored-block transmitter and a Reis knitting-needle receiver, as is claimed to have been done by Mr. Paddock." I presume the italicized statement as above is intended merely as an expression of your own belief, since you can hardly speak with such positiveness for the very numerous experimenters of Germany.

As regards the statement made by me in the article in the January number of the JOURNAL OF THE FRANKLIN INSTITUTE, viz: "Although the author has not personally repeated the experiments of Mr. Paddock with the original apparatus of Reis, yet he is disposed to give full credence to the statements of Mr. Paddock, because he (the author) had, prior to the publication of Mr. Paddock's

letter, held an almost uninterrupted and quite intelligible conversation, continuing for some five or six minutes, in which questions and answers were given and received, by the use of a transmitter, which was an exact reproduction of the Reis bored-block transmitter, and employed the platinum contacts found on the Reis apparatus; and also because he does not see any essential structural differences between this form of transmitter and that in extensive commercial use to-day by the Bell Telephone Company."

Concerning the above, you say: "It is plain, assuming the correctness of the statements, that while using a Reis transmitter, you MUST have used a Bell Telephone RECEIVER. You cannot well venture to deny this fact, although you ignore it in your article." Permit me to suggest that this statement by you seems to me to prove conclusively that you have failed to appreciate the real question at controversy. If you will read the title of my paper of January, 1887, you will see that it is: "Can the Original Reis Telephone *transmit* Intelligible Articulate Speech." The entire question as a matter of invention is limited to the *transmission of speech*. I did use a Bell telephone receiver, but that has nothing to do with the question as to whether the apparatus, shown in *Figs. 1*, and *2*, of my article, and in *Figs. 1*, and *2*, of your open letter, are capable of *transmitting* articulate speech. The controversy is as to whether certain forms of Reis transmitters can transmit speech. You argue that they cannot, because you allege they produce interruptions or breaks in the circuit. I argue that they did, can, and will transmit speech. So far as this question is concerned, what matters it as to the particular form of receiver employed?

The following statement is made by you in reference to the tension of the membrane as described in Mr. Paddock's letter, viz.: "But more remarkable is the story of Mr. Paddock, which you, a scientific physicist, adopt, that he obtained the required tension of the membrane in the wood-cube telephone of Reis through the action of *sunlight*. What an imputation is involved in producing such scientific nonsense before the physicists of the whole world. Do not the two gentlemen know that every animal membrane is very hygroscopic, and that in speaking against such a membrane the moisture of the breath is absorbed, and the membrane must loose its tension, even though Mr. Paddock let the sun shine in his mouth while using the apparatus." The above is such a remarkable illustration of your apparent failure to understand the meaning of a plain statement, that I will rest satisfied with clearing myself before "the physicists of the whole world" by *simply quoting the only remarks* in Mr. Paddock's letter in which any reference to sunlight is made.

Mr. Paddock says on this point:—

"The requisite tension of the membrane I have found to be necessary as Dr. Stein remarks. But in the absence of any mechanical means, such as is furnished by the later forms of the Reis apparatus, I have obtained the desired tension of this membrane by exposing the instrument for a short time to a dry and warm current of air, or still better, to the direct rays of the sun, without detaching the membrane in any way from the wooden block to which it is secured. Under these conditions any degree of tension can be obtained

which is desired, and care only has to be exercised that this tension shall not be *too great!*"

I must take very decided exception to your charge of my suppressing facts—you say: "How is it that in your criticism of my communication you suppressed the fact that Mr. Paddock changed the Reis apparatus and did not experiment on the basis of the mode of the inventor, Ph. Reis." If Mr. Paddock made any such changes as you claim, or any changes at all, I was and still am entirely ignorant of the same, beyond what you state, since I have not at hand the report of the Overland case to which you refer. I will, therefore, leave it to Mr. Paddock to answer you in this regard, if he should think it necessary.

In a subsequent paragraph you say: "I will in your favor and to your honor believe, that you had no knowledge of *the changes which I made* on the original bored-block telephone, which went to the United States through the agency of Prof. Silvanus P. Thompson, of London." I note the necessity which has again arisen for an explanation on your part as to the correct meaning of the portion of your letter above, which I have italicized, and the very careful explanation you have given in the *Electrician and Electrical Engineer*, for September, 1887, in which you state that the above phrase, as translated for that paper, should read, "I will in your favor and to your honor believe, that you had no knowledge of the changes which *were made* (in America) on the original bored-block telephone, which went to the United States through Prof. Silvanus P. Thompson, of London."

If this is what you mean, then permit me to ask with which of your statements quoted below you wish to be credited, viz.: "How is it, that in your (my) criticism of my (your) communication you (I) suppressed the fact, that Mr. Paddock changed the Reis apparatus;" and, "I will in your favor and to your honor believe, that *you had no knowledge of the changes* which *were made* (in America) on the original bored-block telephone, etc."?

Quite on a par with the lack of courtesy displayed in the above, is your statement that: "The first principle of German science is *truth*." The slur you here attempt to cast on American science is not worthy of being answered.

In conclusion, permit me to say that I am quite willing to have the scientific world pass judgment between us, as to "the manner in which we use our scientific weapons."

EDWIN J. HOUSTON,
Prof. of Physics,
CENTRAL HIGH SCHOOL. FRANKLIN INSTITUTE.

PYRO-MAGNETIC MOTORS AND GENERATORS OF ELECTRICITY.

In the very rapid growth and development of the appliances for the generation and utilization of electricity for practical purposes, the dynamo, considered as a commercial machine for converting different forms of energy, stands almost alone in the history of inventions in the rapidity in which it has been developed to practical perfection as far as efficiency of conversion is concerned, ninety-five and even ninety-seven per cent. efficiency from the belt to the available electric energy being now of common occurrence among the better class of dynamos. Inventors must therefore confine their attention to the improvements in mechanical details, and to the reduction in size and cost of the machine. Regarding the size of the dynamo, our knowledge of the principles involved shows us that there are practical limits, such as the limits to the magnetization which can be generated per square inch of cross-section of core, the limits to density of current in the wires of the machine, the limits to the speed of the moving parts, etc., all of which limits have been reached or nearly reached in the best forms of dynamos. It is safe therefore to say that improvements in dynamos will, in general, be confined to details and will not be of a nature to completely revolutionize the present form, considering it as a machine for converting mechanical into electrical energy.

The dynamo is, however, in most cases driven by a steam engine, which in turn requires a boiler, in order to obtain the energy from the coal, which is at present our chief and cheapest source of energy. The steam engine by itself has a tolerably high efficiency, if this efficiency be determined from the available energy at the pulley, and the difference in the energy of the steam as it enters and as it leaves the cylinder; but as the exhaust steam in most cases contains a very large amount of energy, and as the supply steam requires for its generation the inefficient boiler, the real commercial efficiency of both together is very low, being for small powers scarcely more than five to ten per cent., considering the energy in the coal and that which is actually obtainable from the engine. In the very best condensing engines and most approved boilers, and for

very large powers this may be as high as eighteen or perhaps even twenty per cent., but this high efficiency is obtainable only at a great prime cost which is in many cases prohibitory.

Furthermore, the first cost of the engine and boiler is, even for small electrical installations, a very large part of the total first cost of the plant, besides involving a large cost of attendance and maintenance, as compared with that required for the dynamos. The direction in which the attention of inventors is therefore led in order to decrease the cost of electrical energy, is not so much in the improvement of the dynamos, but to supplant the inefficient steam engine and boiler by a better device. It is evident that if such a device or machine could be made which, involving the generator of the electricity, costs as much as boiler, steam engine (including accessories)and dynamos together, which has the same total commercial efficiency, which requires as much attendance and cost of maintenance, and which involves the same indirect expenses for space, buildings, insurance, water, etc., it is evident that it would be on a par with the present system. If such a device or devices reduces any of these costs, whether directly or indirectly, or if it increases the efficiency, it is evidently superior; while if it far exceeds the present system in cheapness or efficiency, it will obviously supplant the steam engine for power purposes, by reconverting the electrical energy into power by means of electric motors, or by direct conversion of the energy of coal into power by direct magnetic or electric devices. As the first cost and cost of maintenance and attendance of the steam engine and boiler is very great, and the efficiency very low, the object to be accomplished by the electrician, namely, to devise a cheaper or more efficient apparatus, is not a difficult one, the difficulty being "how to do it" rather then "what is to be accomplished." One has only to consider the large coal and machine space in a large ocean steamer, to appreciate the great gain of an improved motive-power.

At the recent meeting of the American Association for the Advancement of Science, held in New York, Mr. Thomas A. Edison called the attention of the public to a very interesting and ingenious device, by means of which he has been able to convert the heat of coal or gas into electric currents by means of magnetism. The paper he prepared is reprinted, among other journals, in the *Electrical World*, Aug. 20, 1887, p. 105, and Aug. 27, 1887,

p. 111; *Electrician and Electrical Engineer*, September, p 356; *Scientific American*, Aug. 27, 1887, p. 128; *Manufacturer and Builder*, September, 1887, pp. 194-210.

The general principle of this machine involves the following well-known laws. That a current of electricity is generated in a wire if it moves through a magnetic field across the axis of magnetism, or, in other words, if it cuts lines of magnetic force. This may be done by moving the wire across the lines of force, as in the ordinary dynamo, or by moving the magnets passed the wire, as in some alternating current machines, or by moving the lines of force across the wire by changing their position from one side of the wire to the other. This latter is the principle involved in the alternating current "transformers," or "converters," as explained in the JOURNAL OF THE FRANKLIN INSTITUTE, July, 1887, p. 76, in which therefore both the magnet and the coil are relatively fixed. This latter is also the method of the induction in the Edison pyro-magnetic generator. The other principle involved is based on the law that when iron is heated to a certain degree, it loses its magnetic qualities, or its power to concentrate lines of force in it. If, therefore, a core of iron wound with a coil of wire be placed lengthwise between the poles of a magnet, the lines of force will be concentrated in it, and if it is then heated, they will no longer have the tendency to pass through the core only, but some of them will pass out from the core where they may be considered to have been "confined," and pass directly between the poles of the magnet. In moving from the position in the core to that outside of the core, they must "cut" the coil of wire, and therefore generate a momentary current in it. When the iron is again cooled, these lines will pass from the outside of the coil back again to the core, thus cutting the coil again, and generating a current in the opposite direction. This will give an alternating current, which may be commuted into one of constant direction if desired by a suitable commutator.

The details of the machine have already been described and illustrated in the journals mentioned above, as well as in others, and it is therefore unnecessary to repeat the description here. It will suffice to state here that the machine consists in general of a core of soft iron made of sheet metal, having a thickness, we are

told, of ·002 of an inch,* and rolled so as to permit hot gases to pass between the sheets; this core is surrounded by a coil of wire, and while the core is in a strong magnetic field, it is alternately heated and cooled, thus inducing currents in the coil.

The regenerating principle of using the current generated by the machine to supply the magnets, has, we are informed, been applied to this machine, as well as the heat regenerative principle, that is the utilization of the waste heat to warm the air used in the combustion of the fuel, thus increasing the temperature of the heat. It is stated that such pyro-magnetic generators are beautifully self-regulating for constant potential.

A further study of the principles involved will show that there are practical limits to the development of such a generator, some of which have been very nearly, if not quite, reached in this machine. The chief one of these is the limit to the speed of cutting lines of force, and therefore a limit to the electro-motive force, or electrical pressure, which can be generated with a given field magnet. It is well known that it is the electro-motive force or electrical pressure, and not the current which is primarily generated by any electrical generator, the current being dependent on this electro-motive force, the size of the wire and the external resistance. This electro-motive force is proportional to two factors, first, to the amount of magnetism, or the number of lines of force, which is practically unlimited, as the field magnets may be made of any size; second, to the speed with which these lines are cut by the wire, which in this machine is dependent on the speed with which the magnetic qualities of the iron core may be destroyed and restored by the heat. This latter has a comparatively low limit in practice, 120 heatings per minute being, we are informed, the fastest rate at which these changes can take place. This means that the useful lines of force of the field magnet can be cut only 240 times a minute, while in the ordinary dynamo, having a speed say of 1,200 revolutions, the lines are cut 2,400 times a minute; from this alone it would follow that the amount of magnetism of a pyro-magnetic generator of this kind, neglecting all other factors, would have to be ten times as great as in a dynamo generating the same potential. Such

* The thickness of the paper of this JOURNAL is very nearly ·003 of an inch.

a generator is therefore allied to a dynamo having a very low speed, and must therefore be quite large as compared to an ordinary high speed dynamo for the same output. Other considerations will modify these proportions somewhat, but as Mr. Edison himself states, the machines of this kind must necessarily be quite large and heavy. Fortunately, however, magnetism is cheap, and the large size and heavy weight of a machine is not always a very objectionable quality; it is outweighed many times by the fact that such a machine is stated to require no more attendance than that required for an ordinary furnace for heating houses.

Another feature of these pyro-magnetic generators, and one which will no doubt present many serious difficulties, is that the iron cores which are to be heated and cooled so rapidly, must necessarily be made of very thin metal, and as it has to be heated to redness to destroy the magnetic qualities, it is evident that rapid oxidation and disintegration of the metal will take place, which will seriously affect the life of those parts of such a machine. This feature may, however, not be an insurmountable obstacle, and opens a sphere for inventive genius or for discovery.

It is evident that the source of the electrical energy generated is the heat of the fuel, and that it takes more heat to raise the temperature of a magnetized piece of iron to redness than a piece which is not magnetized, or stated in technical terms, the specific heat of magnetized iron is greater than that of unmagnetized. The difference is evidently that which may be converted into electrical energy. It is of course not necessary to heat and cool the iron through a greater range of temperature than is required to produce the magnetic change, which range it is said is not very great, and therefore it may ultimately be possible to limit the heating and cooling in such a way that the greater part of the heat actually consumed is in this difference in the specific heats, thus making the efficiency of conversion very high. This also opens a sphere for inventive genius. The difficulties, however, which are encountered here are analogous to those in the ordinary boiler, as the heated products of combustion pass off at a high temperature, thus lowering the efficiency of conversion very greatly.

Although it is difficult to make any predictions at a time, like the present, when inventions and discoveries are so numerous, yet we are probably safe in venturing to say that the great problem of

obtaining electrical energy directly from coal or other cheap sources of energy, will ultimately be accomplished in a different way than by means of such pyro-magnetic generators. Although this is a step in the right direction, it is by no means a complete solution of the problem. The chief alternative methods at present known to us are the thermopile and the battery. While the future of the former does not seem to be promising, the latter has already made considerable progress, chiefly in the form of batteries consuming heat or some cheap fuel, such as gas or carbon.

In the history of the development of the pyro-magnetic generator, we are pleased to be able to record another instance, in which the germ of a great invention or discovery was first brought before the public in the FRANKLIN INSTITUTE. The chief underlying principle of the pyro-magnetic generator of electricity is that of the pyro- or thermo-magnetic motor, which, it appears, was invented by Profs. E. J. Houston and Elihu Thomson, at that time professors in the High School of this city, and first described by them in the JOURNAL OF THE FRANKLIN INSTITUTE, for January, 1879, p. 39, as follows:

A CURIOUS THERMO-MAGNETIC MOTOR.

By PROFS. EDWIN J. HOUSTON and ELIHU THOMSON.

During investigations by the authors, concerning the increase in the coercitive force of steel by changes of temperature, the following curious thermo-magnetic motor was devised. This motor, though devoid of practical value, will, no doubt, be of sufficient scientific interest to warrant a short description.

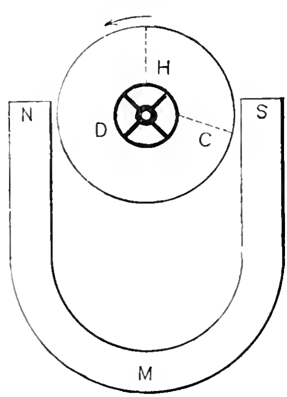

FIG. 1.

In *Fig. 1*, a disc or ring of thin steel, *D*, is mounted on an axis, so as to be quite free to move. The edges of the wheel are placed opposite the poles *N* and *S*, of a magnet. In this position the wheel, of course, becomes magnetized by induction.

If now, any section of the wheel, as *H*, be sufficiently heated, the disc will move in the direction shown by the arrow. The cause of this motion is as follows: The section *H*, when heated, has its coercitive force thereby increased, and being less powerfully magnetized by the induction of the pole *S*, than the portion *C*, immediately adjacent to it, the attraction exerted by the pole *S* on the latter portion is thereby sufficient to cause a movement

of the disc in the direction shown by the arrow. If a constant source of heat be placed at *H*, a slow rotation in the direction shown is maintained.

To ensure success, the disc must be sufficiently thin to prevent its acquiring a uniform temperature. If the source of heat be at the same time applied at diametrically opposite portions of the disc, as at *H* and *D*, adjacent to the poles, the same effect will be produced. Since the amount of heat expended in producing motion of the disc is so enormous when compared with the force developed, it will be readily understood that this motor is of no value as such, but must be regarded as an interesting example of the inconvertibility of force.

Expressed in more modern terms, the explanation of the rotation of the disc is that the heating of the two diametrically opposite parts, thus demagnetizing them, acts as though these parts of the disc were cut off, leaving an inclined piece of magnetic iron, which, therefore, receives a more or less tangential pull by the shifted lines of force to bring it into the direction of the axis of magnetization of the field.

It will be noticed that in both the motor and the generator the general principle is the same, namely, that the demagnetization of iron by heat is used to change the position of the lines of force; in the motor, this change of position is made use of to turn the iron armature on its axis, and in the generator the change of position of the lines is made use of to induce a current by causing the lines to "cut" the wires of a coil. The difference between the two is, therefore, not analogous to the difference between the ordinary electric motor and dynamo, of which one is the exact reverse of the other; in the pyro-magnetic generator there is a new element, namely, the electric current, which is entirely absent in the motor.

The next reference to a pyro-magnetic motor appears to be the following description, published five years later, of a motor which is precisely the same in principle, though evidently less efficient than that of Houston and Thomson. It is taken from *Science*, vol. iii, 1884, March 7th, No. 57, p. 274.

A NOVEL MAGNETIC ENGINE.

It is a well-known fact that iron, when heated to a red heat, ceases to be magnetic; so that an armature, after being heated to redness, may be removed from its magnet by the expenditure of only a small fraction of the energy which is developed by the attraction of the same armature when it has cooled.

Manifestly, this fact might be employed in the construction of a motor, which, while of no practical value, is of theoretical interest, in which a per-

manent magnet should act as the direct motive force. This has been done in the following manner: In *Fig. 2*, *a*, *b*, *c* represents a ring thirteen centimetres in diameter, and supported horizontally upon radial arms and an axis of some non-magnetic metal. This ring is made of one or more turns of iron wire of about a millimetre diameter. *N S* is either a permanent or an electro-magnet. The axis is furnished with a driving pulley, cord and weight, as shown in the figure.

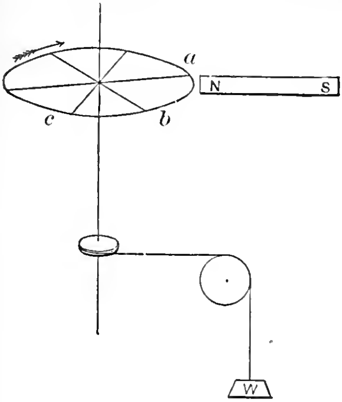

FIG. 2.

That part of the ring which lies between *a* and *c* is heated to bright redness by means of two or three Bunsen burners. The magnet then exerts a preponderating attraction upon the farther or cool side of the ring, and the latter revolves as indicated by the arrow. As fast as the ring enters the space *a*, *b*, *c*, it becomes red-hot and non-magnetic, and a lack of equilibrium is thus maintained, which results in a continuous rotation.

The motion is necessarily quite slow on account of the considerable time required to heat the iron ring. In the actual experiment, moreover, considerable difficulty was experienced from the distortion which the ring underwent when softened by the heat, in consequence of which the speed of rotation became very irregular. With a permanent steel magnet, a speed of about one revolution in two minutes was obtained; and with a powerful electromagnet, a weight of six grams was raised fifty centimetres in six minutes, and in a second experiment, the ring having become quite distorted, ninety centimetres in thirty minutes.

Of course, the source of energy is the Bunsen burners; and the experiment leads at once to the fact that the specific heat of magnetized iron is greater than that of unmagnetized. CHAS. K. MCGEE.

University of Michigan, Ann Arbor, February 19, 1884.

It will be noticed that this is not only no improvement of the earlier form, but as the field is not a closed one, it is evidently not as effective nor as efficient. It is nevertheless a very ingenious invention and appears to have been made independently of the earlier form.

Next in the order of date appears to be the invention of a pyro-magnetic generator by Emile Berliner, and called by him an "electric furnace generator." The following is a copy of his application for a patent, filed June 18, 1885.

ELECTRIC FURNACE GENERATOR.

[*Filed June 18, 1885.*]

SPECIFICATION.

To all whom it may concern:

Be it known that I, Emile Berliner, citizen of the United States of America, residing at Washington, in the County of Washington, and District of Columbia, have invented certain new and useful improvements in Electric Furnace Generators, of which the following is a specification, reference being had therein to the accompanying drawing.

The invention has for its purpose the production of electricity by the assistance of heat, and it is based on the following principles:

Iron, when heated to bright red, is not affected by magnetism, and in turn does not react upon that force; but if cooled down to a dull red heat it instantly regains the ability to absorb or engage magnetism. Further-

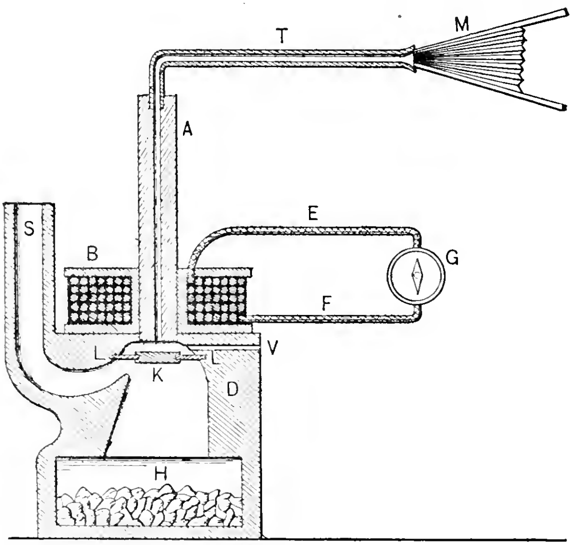

FIG. 3.

more, if a magnet is capable of holding suspended a mass of iron, *i. e.*, an iron armature weighing not more than, say twenty pounds, it will also be capable of holding suspended on it a mass of brass, or other non-magnetic material weighing, say fifteen pounds, by means of an iron armature weighing only a fraction of an ounce.

This shows that in order to engage, say three-quarters of the attractive capacity of a magnet, a very small portion of the equivalent weight of iron is required to do this. It is also well known that the greater the amount of displacement of the magnetic field in a magneto-electric generator, the greater is the current produced in the conductor of said generator.

If now I take a magnet and provide it with a coil around its pole or poles, and place before this magnet and in proximity to the coil a piece of iron

heated to bright red, nothing will occur to disturb the magnetic field, but the instant the iron cools down to a dull red the magnetism becomes excited, and a momentary current of electricity is produced in the coil.

I may go a step further and have a series of such magnet coils and iron armatures, and by connecting the coils into the same circuit and cooling the armatures in rotation, one after the other, a number of electrical impulses will be produced which, when they follow one another rapidly, will approximate a continuous electric current.

These being the underlying principles of my invention, which, of course, is capable of being worked out into different devices for the accomplishment of the same purpose, I will confine myself in this specification to a simple apparatus, capable of demonstrating the underlying principles heretofore stated. (*See Fig. 3*).

In the drawing, *A* is a magnet, having a tubular hole through its centre. *B* is a coil of insulated wire, the insulating material being fire-proof, like asbestos, cement or other fire-proof substance. *D* is a brick furnace provided with a fire-box *H* and chimney *S*. *K* is a plate or disc of iron, set inside a brass ring *L*, which in turn is so imbedded in the upper part of the furnace that the flames cannot strike above it, but will heat it only from below. The terminals of the coil, which are *E* and *F*, are closed through the galvanometer or other electrical apparatus *G*. A tube *T* is inserted into the top of the magnet and is provided with and connected to a pair of bellows *M*.

In order to produce electricity, a fire is started in the box *H*; the heat on its way to the chimney must then impinge against the iron armature *K*, which under the continuous influence of the heat, will soon be rendered bright red, when it will have lost its power to engage the magnet *A*.

If now the bellows be compressed, a current of cooler air will be forced through tubes *T* and *S*, against the plate or disc *K*, and out through the vent holes *V*. As soon, however, as the air strikes the plate *K*, it will cool it sufficiently to reduce the bright red to a dull red, and instantly the magnetic influence will be re-established, and a corresponding change in the magnetic field will be accompanied by an electric impulse in the coil *B*. If now the current of cooled air be discontinued or averted, the plate will again resume the bright red, and a current in the opposite direction, but of less intensity, will occur in the coil *B*.

It is evident that a working up and down of the bellows at a slow enough rate will alternately change the bright red to a dull red and the reverse, and the consequence will be a series of electrical impulses in the coil *B*, which may then operate the electrical apparatus *G*.

It will be observed, since the depression of the bellows requires comparatively little force, that the principal acting force is the heat, which suspends the magnetic influence between the magnet and armature, while a little draft of compressed cooler air re-establishes this influence suddenly. The current thereby produced might be utilized to charge another coil surrounding the magnet and re-enforce the magnetic field; and in that case the magnet might be substituted by a tubular core of iron; the bellows might be dispensed with, and a constant source of compressed air might be employed, regulated by

suitable mechanism to supply the air puffs to be directed against armature *K*; or even pieces of cold metal might be made to approach the armature at intervals, and cool it by contact with the same; or by absorption and radiation; or a series of coils and magnets might be placed toward one larger armature disc forming a common armature, heated by one furnace.

All these ideas can be brought to bear on the subject whenever time offers itself to experiment, which is the most effective modus. For the present, I claim:

The combination with the magnetic field and a conductor situated therein, of a piece of iron which is alternately heated and cooled, thereby producing electricity, substantially as described.

In testimony whereof, I have affixed my signature in the presence of two witnesses. EMILE BERLINER.

CHARLES W. HANDY, } Witnesses.
MYER COHEN. }

It will be noticed that this appears to be the first suggestion to use this principle to generate or induce an electric current, and, therefore, this ante-dates the publication of the description of Edison's invention of practically the same thing, by over two years. While Edison's form of the generator is unquestionably a very great improvement of this form, being more practical, effective and efficient, it involves no new principle, but differs only in details, which, however, are evidently very important ones, and which add to Mr. Edison's well-earned fame for his ability to reduce mere suggestions or laboratory experiments to practical devices, in which form they may be of use to the general public.*

CARL HERING.

NOTE.—Since writing the above, my attention has been called to a communication by Wm. B. Cooper, of Philadelphia, to the *Electrical Review*, New York, September 24, 1887, in which he states that he devised a motor of this kind in 1884–85, which he brought before the FRANKLIN INSTITUTE at that time. He also suggested the idea of generating electricity by this principle. He was very much discouraged, however, by the fact that the iron oxidized very rapidly. He also refers to an article by D. Gore, *Philosophical Magazine*, vol. xl, 1870, p. 173, in which is described an electrical *generator* based on this principle, and which, therefore, anticipates the idea of Berliner and Edison. In Gore's apparatus there was a coil at each end of a bar of iron, a battery current was made to flow through one, and the other was connected to a galvanometer; on heating and cooling the bar in the middle a current was indicated in the galvanometer. C. H.

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* The JOURNAL is indebted to the *Electrical World* for the use of the engravings used in this article.—THE COMMITTEE.

On FLINT'S INVESTIGATION of the NICARAGUAN WOODS.

By R. H. Thurston, Director of the Sibley College of Cornell University.

[*Presented to the American Association for the Advancement of Science, New York Meeting, August, 1887.*]

Preliminary Remarks. By R. H. Thurston.—It had long been a conviction of the writer that the tropical and semi-tropical countries of America possess a large number of valuable timber trees of which nothing is known by our engineers or builders, but which, for some of the more important purposes, as well as for ornamental and exceptional constructions, may have extraordinary value. As the process of stripping our own land of timber goes on, and as our pines and finer grades of timber trees become gradually more and more difficult and costly of procurement; the necessity will become more and more pressing of going into the more heavily forested countries nearer the equator for our supplies of lumber. It is thus becoming continually more important that we learn something of the resources of such countries, and especially of the useful qualities of the woods available for building purposes. A knowledge of the extraordinary strength, and of the other valuable properties of some of these timbers, will unquestionably, in time, lead to the opening of our markets to them, and to a trade that may prove to be of inestimable advantage to both purchaser and vendor. A few years more will see the great forests of the Northwest stripped of their best timber, and the supply will then be mainly drawn from the South and the Pacific Coast; but the enormous rate of growth of the country in population, and still more in manufactures, will sweep those districts clean at a rapidly increasing rate of destruction, and it will be found, probably much sooner than is now generally imagined, that we must look elsewhere for the enormous supplies demanded by our markets. The growth of this demand is a geometrical one.

That most remarkable of recent works on economical subjects, that romance of statistics, *Triumphant Democracy*, gives some striking facts abstracted from the last census. Mr. Carnegie finds

that our lumber trade has quadrupled within the generation, the business now employing about 150,000 men, and producing nearly $250,000,000 worth of lumber. About $40,000,000 is invested in this trade in Michigan alone, and the three states, Michigan, Minnesota and Wisconsin cut, in the year 1880, over 7,000,000,000 feet of timber, in the form of marketable lumber, besides millions of feet in the shape of railroad ties and other minor products. It is estimated that the pine forests of the northwest will be gone in about twenty or twenty-five years; but the southern forests are of much greater extent. It is not certain that we shall very soon be entirely deprived of the light and comparatively soft growing woods; but their extinction would seem to be but a matter of time, and, as the lives of nations are counted, not a long time.

Mr. Jessup has collected for the New York Museum of Natural History some 400 varieties of North American woods, including very many which are of very great value for constructive purposes; some of these, as the sycamore, have not been as yet much used, but are now being found to have peculiar value for special purposes. The consumption of these hitherto neglected timbers will continually and rapidly increase. Others, as the live oak of our Gulf States, have been used exclusively for special purposes, as in ship-building, and have, by the progress of improvement in the art, been thrown out of use, to come in again at some later time in a way as yet unforeseen. The soft woods are those which, from their soundness and especially from the ease with which they can be worked, are in greatest demand, and, fortunately, they are those which have hitherto been most plentiful and cheap in our markets; but they are gradually becoming less accessible and more difficult of procurement: their cost is thus threatening to become seriously increased, and we are likely to be compelled to find ways of substituting the hard woods for the soft in our constructions. A century ago, oak was the most common of building timbers in the older countries of the world; it seems now possible that it, and the other hard woods, may, in time, again come into general use; but, if so, we must, after a time, go into the southern and neighboring states for such woods. Once we are brought to the use of such hard woods again, we shall, perforce, be brought to the construction of buildings capable of withstanding the teeth of time as effectively as did those of our ancestors of the middle ages.

We are to-day sending out into the markets of the world, annually, something like $30,000,000 worth, probably, of wood, in the form of manufactured articles mainly, particularly as furniture, and the time is coming when we shall supply a very large part of the world with its timber and manufactured wood products; but this will only hasten the day when we must look to the West Indies and to Central and Western America, perhaps to South America, for our own supplies

It was considerations such as have been above outlined that led the writer, some years ago, to endeavor to secure such data relative to the useful qualities of the tropical and semi-tropical woods as would, in the course of time, prove useful to our own people as well as to the citizens of those neighboring countries to which we shall be likely to first look for our supplies of the heavier sorts of timber; for it will be seen that nearly all of the semi-tropical and tropical woods are of the hardier varieties and are distinguished rather by their hardness and strength than by lightness and ease of working—the peculiar qualities of the varieties of the coniferæ from which we obtain the greater part of our timber to-day. The first attempt to investigate these hard woods of the warmer latitudes in a systematic and satisfactory manner was probably that undertaken some ten or fifteen years ago by French engineers and naturalists studying the trees of New Caledonia. Later investigations of a similar character have been made, usually since the work directed by the writer about to be referred to as initiating his own researches, by British authorities, in India and in the Australasian colonies of Great Britain. The first systematic study of these classes of woods in America, so far as the writer has observed, was made at the suggestion and request of the writer by Mr. E. D. Estrada and published in Van Nostrand's *Magazine* for November and December, 1883. Recently, another investigation has been undertaken in the Mechanical Laboratory of the Sibley College, Cornell University, by Mr. Rufus Flint, the results of which will here be presented. Mr. Flint, although by descent on the father's side an American, is a native of Nicaragua, and, until coming into the United States to obtain his education, has been a resident of that country. Through his relatives and friends and assisted by the liberality of the Government, which allows such material to enter duty free, Mr. Flint has been able to secure a fine collection

of the woods of Nicaragua, and thus to enter upon a research as important in its bearing upon the business interests of his own country and of the United States as that conducted by Mr. Estrada.

Examining the data thus secured by these investigators, we find that the tropical woods are distinguished by their extraordinary strength, elasticity, beauty of grain and durability. The few already known to us, such as mahogany, rosewood and some of the cedars, may be taken as illustrations of the several principal classes of timbers to be found in the forests of Central America and the Islands of the Caribbean Sea. These woods are all coming into demand very rapidly already, for house decoration, and in the construction of the finer grades of furniture, and an examination of the magnificent collection gathered at New York, by Mr. Jessup, will reveal the fact that we have but begun to make application of the enormous variety of woods which are readily obtainable and available for such purposes. These statements will be seen to be true of the woods of Central America here to be described, as well as of the Cuban woods already reported upon. In both cases, but a few of an immense number of woods have been taken for investigation; but these selected samples may be taken as illustrative of the whole product of this vast arboretum. The tropical trees attain enormous sizes, are extraordinarily solid, close and firm of grain, excel in the beauty and variety of their coloring, in fineness of texture, and especially in their wonderful durability, whether exposed to the corroding influence of the atmosphere, to the action of heat and moisture, or to the attacks of insects. Ironwood and lignum vitæ exemplify the first of these characteristics; mahogany, rosewood, tulipwood, and others, illustrate their beautiful color and grain; and the live oak and teak are good examples illustrating their power of resisting the action of oxygen and the attacks of the teredo and of the limnoria.

The investigations of the Central American woods were made in the several testing machines and the workshops of the Sibley College, and the results were reported to the writer in a paper presented in June last, the substance of which is here given. The report is so well written and so complete that it has been thought best to give the whole in the words of the observer. The figures have been very carefully checked, and are believed to be perfectly

reliable throughout. The machines used had been standardized and were known to be practically exact, and it is thought that the data thus obtained, as here given, may be of real value to the profession, as well as to the two countries most nearly interested in the results of the research. The native nomenclature is given throughout. It was found difficult to obtain the botanical names of all these woods, and, in many cases, those obtained were subject to some question in consequence of the fact that the botanists seem to have had comparatively few opportunities of study of these woods; but the introduction of the timbers of these regions into our own markets will undoubtedly lead to the adoption of the nomenclature obtained in their homes, and no inconvenience will arise, it is thought, from the omission of the technical names.

The examination of the Cuban woods, to which reference has been made, showed moduli of elasticity as deduced from the experiments on transverse stress and strain, varying from 1,500,000 to 2,500,000, British measure, the great majority of the dozen and a-half varieties studied ranging above 2,000,000, or equalling the stiffness of the Indian teak, and exceeding that of any known variety of our native timber, with the exception of an occasional sample of the strongest and stiffest of the choicest of Southern long-leaved, yellow pine. The figures for the best of the Cuban woods are above those of any known North American woods, exceeding their best figures by fifty per cent., nearly. Three-fourths of them are stiffer than the best teak. The moduli of rupture by transverse stress vary between 15,000 and 20,000 pounds per square inch, as a rule, in but one case falling to 8,000, and in several exceeding 20,000, the average being not far from 18,000. The densities average above that of water, and many samples exceed that unit by some twenty per cent. Quite a number of these woods have just the qualities which distinguish yellow pine of the finer grades; for example, Baria (*Cordia gérascanthoides*) and Caobilla (*Crotos lucidam*), as representing the lighter varieties, and Majagua (*Paritium elatum*), an even lighter and stronger wood; Dagame (*Colycophyllum candidissimum*), one of the most common of West Indian woods, weighing but fifty-six pounds per cubic foot, and having a modulus of rupture of 16,000 pounds.

Sabicu (*Mimosa adorantissima*) has just the weight of water, or a trifle less, and, with a modulus of elasticity of about 2,400,000,

exhibits a modulus of rupture exceeding 20,000. A dozen of these varieties are higher in rank than the best building material found in our native forests.

The Central American woods, growing on a more widely distributed area, in a soil of less uniform character and in a greater variety of climate, are naturally of more widely differing character than those of Cuba. The high and dry atmosphere and more innutritious soil of the interior, and the rich bottom lands of the swampy regions of the coast, with every intermediate condition of soil, climate and exposure, produce timber of soft as well as hard varieties, of less as well as of greater strength or elasticity, and thus yield to the market a larger assortment of useful woods than could any insular district. The moduli of rupture, as determined by Mr. Flint, vary from 7,000 to nearly 30,000, and the moduli of elasticity from about 600,000 to about 2,500,000, usually approximating 2,000,000, the modulus of rupture commonly falling at about 22,000.

INTRODUCTION BY RUFUS FLINT, M.E.

The woods of Nicaragua, grown under the sunny sky of the Torrid Zone, and in the mountains, valleys and on the sandy shores of the Atlantic and Pacific Oceans, are many in number and of widely different character in nature, strength and color. Unrestricted in their growth, in the exuberant and wild forests and woods of the country they attain enormous sizes, and exhibit great strength and solidity.

As a general rule, they are of delicate hues and beautiful colors, exhibit extreme fineness of grain and have marked peculiarities in texture and general appearance.

The investigation of their strength and natural properties is a matter of great interest. With the facilities offered in the Mechanical Laboratory of Sibley College for complete investigation of the materials of construction, I have thought it advisable to conduct such an investigation of a few of the Nicaraguan woods so as to make known the characteristics, not only of those that have found their way into the markets of this country and of Europe, mainly because of their worth as dye woods or for ornament, but also some others, perhaps of better if not equally prized properties, which have yet remained unknown in the industries.

In undertaking this investigation, I hope to find results which may prove of value and interest to the artisans of my native land, and also aid in developing, to a certain extent, the commerce and industries of Nicaragua, by making some of its natural products known in the United States and Europe.

I have restricted myself to the study of those woods which are most used in engineering construction and decoration, leaving out the dye and other woods, which, perhaps, may be of equal interest and value, there being quite as many unknown, or at least unused, valuable woods and plants of this latter kind.

I was encouraged from the start by Prof. Thurston, who very kindly wrote to the Government of Nicaragua, asking for a collection of the most important woods of the country, expecting that its officials would take, or at least show, some interest, and thus secure a good collection of the woods. But unfortunately we were in this disappointed. It was by my father's interest, kindness and persistence, and through my friend Miguel Ugarte's active and courteous help, that I was supplied with as good a collection as could be obtained in the short time allowed to collect them. Steps were taken to get the woods, seasoned and sound, from persons engaged in working them, but they failed to obtain them. It is for this reason that the woods tested have not been entirely satisfactory, as they were cut green, and the men could not in most cases fell large trees to get the heart or the best of the wood; as a consequence, they checked on the way, and many of them were found to be knotty. The woods were collected in one month, within a circuit of three leagues, on the hills about Belen in the agricultural and chocolate-raising "Departmento," of Rivas, between Lake Nicaragua and the Pacific Ocean. Some few, obtained from a carpenter, are seasoned. My collection of fifty different varieties represent about half the number of the useful woods of the country.

The terms used may be thus defined: The modulus of rupture for tension is the force necessary to pull asunder a bar whose section is one square inch, when acting in the direction of the axis of the bar.

The modulus of rupture for compression is the pressure necessary to crush a piece of any material whose section is unity and whose length does not exceed about five times its diameter.

The modulus of rupture for transverse stress is the stress at the instant of rupture upon a unit of the section which is most remote from the neutral axis on the side which first ruptures.

The modulus of elasticity is a value which expresses the relation between the extension, compression, or other deformation of a bar, and the force which produces the deformation.

Resilience is a measure of the capacity of a material to resist shock, and its value is equal to the amount of energy expended, or the "work" performed in producing distortion or rupture.

GENERAL DESCRIPTION OF THE WOODS.

(1.) *Carbon.*—Extraordinarily solid, equalled only by the Piedra and the Quiebra-hacha. Is almost imperishable when used for posts, and is supposed to be very good for railroad ties. The tree attains a height of 30 feet, and measures 12 inches in diameter. Is common in most of the wooded districts. The wood is of a very fine grain, with peculiar dark streaks; very much like the mahogany in appearance and in color, but heavier and much handsomer. Is easy of working, and turns very smoothly.

(2.) *Cedro* (Cedar.)—The wood, on account of its peculiar properties, has found a place in all the markets abroad. In Nicaragua, it is found abundantly, and attains enormous sizes. Is used extensively for furniture, frames, book cases, etc. It is even used by the Indians for boats, which they work out entire from the trunk of the tree.

(3.) *Chaperno.*—Dark red color, turns and planes very smoothly. It has a fine grain and great strength and is susceptible of high polish. It is used extensively for cross pieces of drawers and tables. Is extremely durable. There are two varieties of this wood, black and white, as they are called respectively. The tree attains a height of forty feet.

(4.) *Chiquirin.*—Dark yellowish wood with a strong cedar smell. Is heavy, fine grained and planes smoothly. The tree attains a height of thirty feet, and one foot in diameter. It has various uses, is durable, and having odor is probably not subject to attacks by insects.

(5.) *Cortez.*—Extremely heavy and very fine-grained, of a very dark yellowish color. When broken in splinters, it gives off a fine yellow powder, which has similar properties to litmus; it turns

bright red when mixed with soap water. It is a large tree like the Nacascolo. It is used in cabinet work, for framing, etc. The only place in which I can remember to have seen it growing is on a rocky hill at the foot of the volcano Mombacho, near the shore of Lake Nicaragua. The hill is covered with these trees, which, in the beginning of every spring, are a beautiful sight, being covered with yellow flowers.

(6.) *Granadillo-negro.*—Wood very much esteemed for interior decoration, on account of its handsome dark color, fineness of grain and ease of working. The tree attains a height of thirty feet, and is found on the shores of the rivers which flow into the eastern side of Lake Nicaragua.

(7.) *Gauchipilin.*—Fine grained wood of a light yellowish color, heavy and tough. The tree attains a height of 30 or 40 feet, and has a diameter of 15 inches. It is irregularly branched. Much used by the artisans for durable work, as it resists moisture for years. It is also used for telegraph poles and railroad ties. Is abundant all over the country.

(8.) *Guapinol.*—The tree is nearly as large as the Jenisero, and its branches large but more erect. The wood is of a light mahogany color, long grained, but very compact, heavy and tough. Is used almost exclusively for cylinders of sugar cane mills, while the teeth moving them are made of guachipilin, guayacan or other similar wood.

(9.) *Guayabo de Monte.*—Attains a height of sixty feet, is of irregular diameter, and seldom over two feet above the lateral roots, acting as braces, which support the trunk. It has a fine grain and is very tenacious. The test made probably does not show full strength, as it was an inferior sample. It is used for small masts and the weather streaks of boats. According to Mr. D. L. Murray, who preferred it above all others for launch guards, it resists wear and tear better than any other wood.

(10.) *Guiliguiste.*—A wood unknown to commerce. Has a light brown color in the heart, is fine and fibrous of grain. It ran above the average in compression. It is not as durable as the other hard woods, but from its beautiful grain and color, and from its ease of working, it would seem that it should be used for interior house work. The tree is small, growing only about 30 feet high, with a diameter from 15 to 18 inches.

(11.) *Jenisero.*—One of the most useful trees, and one of the largest in the country. Attains a height of 90 feet, with 7 in diameter, and its large branches cover a space of over 100 feet in diameter. In Nagarote, a town in the Departmento de Leon, at the junction of one of its streets with the large road from the western departmentos, there is a Jenisero whose branches cover a circumference of 348 feet (about 9,498 feet area); it is 90 feet high, and has a circumference of 21 feet at 4 feet from the ground, according to Senor F. Guerreo Baster.

The wood has a light to dark color, and a peculiar grain; it is open and wide in the annual rings, but very compact between. It is used for cart wheels, lasting for years without tires on clay soil. Used also by the carpenters in various ways. Its fruit is eaten by the cattle, and is used to sour the milk. It is fairly well distributed over the country.

(12.) *Jicaro-Sacaguacal.*—Attains a height of 20 feet, and a diameter from 10 to 12 inches. Common on marshy land. It is of a nearly white color and very tough, used in saddlery and for boat-knees. Resists moisture, and is durable in salt water. The shells of its fruits, after being worked, are used by the natives for drinking vessels. They carve them very beautifully and artistically.

(13.) *Laurel.*—Dark color, light, strong and elastic wood and very easy of working. There are two varieties, male and female, as they are popularly called. Used mostly in frames for cots, and for work where elasticity is required. The dark kind is preferable. Both have a spicy smell. The tree attains a height of 40 feet, and a diameter from 8 to 12 inches, seldom over 8.

(14.) *Lhgualtil.*—This is one of the trees having many peculiar natural properties. Its fruits have a rich fragrance and flavor, and when green give out a coloring substance. From its bark a bluish and sometimes a purple substance is obtained, and from its sap thirty per cent. sugar may be obtained; is one of the most elastic woods found in Nicaragua. It is used for drum hoops, canes, etc. The tree seldom attains a height of twenty feet, and about twelve inches in diameter.

(15.) *Madera-Negra.*—One of the most useful trees found in Nicaragua, not only on account of its durability, strength and excellency for fire wood, giving out intense heat; but also from its method of growth. It is about the only tree used to shade the

chocolate trees. It has a rapid growth, and is early produced from the seed. Mostly used for railroad ties, posts for houses, fence posts, foundations, etc. Has a dark, yellowish color in the heart, is fine grained, heavy and tough. It grows with oblong cavities wasting a good deal of the wood when being dressed. However, straight logs, 1 foot square and 30 feet high can be obtained.

(16). *Madrono.*—There are two kinds, white and dark. It has a fine grain, and is heavy. Its strength may be seen from the tests in torsion and by transverse stress. Its growth is irregular and branching.

(17.) *Mahogany.*—This is too well known to demand description. In Nicaragua it is fairly well distributed. The best and most valuable is exported to a considerable extent from the Mosquito territory, where it grows abundantly, and to its fullest size. It is also found along the Pacific Coast in considerable quantities

(18.) *Moran.*—Solid and fine grained wood of a beautiful yellow color. After it has been turned, it looks as if it had been polished; planes very easily. It is exported in great quantities as a dye-wood. Is often used for columns. Attains a height of from 30 to 35 feet. and a diameter of from 12 to 18 inches.

(19.) *Nacascolo.*—The wood is extremely heavy, very fine grained, and of a handsome dark color. Its toughness is shown by the test in torsion. The fruit is known by the names of *Nacascolo* or *Dividi*, and used for dyeing purposes when dry. It is one of the largest of hard-wood trees. Its trunk, although irregular in growth, and seldom over twenty feet to the point where it branches, is 6 feet in diameter. It attains a height of 60 feet, and is found more abundantly on the Atlantic Coast. Is an excellent wood for railroad ties.

(20.) *Nancite.*—Has a soft pink color, fine grain and works very easily. The tree is small, grows on arid hills, and seldom attains 30 feet height. Its bark is used for tanning, and its fruits are to the Nicaraguenses what cherries are to the North Americans.

(21.) *Nispero.*—It may be said that there are two kinds, wild and cultivated. Large fruit tree of a thick and handsome foliage. The trunk is straight and free from limbs. The tree attains a height of 60 feet and a diameter of 2 feet. It is abundant all through the country in farms near the towns and in the wild

forests. Exclusively used for wharves, bridges and posts. Resists moisture equal to any of the hard woods, and is said to petrify in the water. The wood is of an exceedingly fine grain, has a beautiful red color, and is very easy working It behaved the best of any under compression, bulging out considerably before showing any sign of shearing or split.

(22.) *Oja-tostada* —Has a very light pink color, is fine grained and light. It is one of the most elastic and tough woods tested, as may be seen from the tests by transverse stress and by compression. It is very good for light and strong constructions.

(23.) *Palo de Arco.*—The sample tested planed easily in parts, and in parts less readily, probably on account of being green. Is hard, has a fine grain, and a light red color. It is used for construction where easy of access. It grows along the coast, and in the coast range of mountains, attaining a height of 30 feet and 15 inches in diameter.

(24.) *Piedra.* (Stone.)—Has a fine grain, is heavy and strong, of a yellowish color in the sap, and deep red in the heart. Turns very smoothly and is one of the hardest and heaviest woods known and yet not difficult to turn or plane. The tree attains a height of 40 feet, and has a diameter of from 15 to 18 inches. It is imperishable. Used in many places for pillars and transverse beams of houses on farms distant from towns where it is easy of access. Is abundant on the hills along the coast on the Pacific slope. There are two varieties. It is an excellent wood not only for interior decoration or for heavy furniture on account of its beautiful color and fineness of grain, but also for heavy constructions, as for foundations for engines or heavy machinery.

(25.) *Pochote.*—Tree of enormous dimensions. The wood is similar to the cedar, but much softer. It is used, however, in housework for doors, walls, floors, shingles, etc.

(26.) *Quiebra-hacha* (axe-breaker). There are two kinds, red and black. The latter, which was tested in three ways, is a most wonderfully tough wood. It has a color and appearance like the black-walnut, and its grain is similar to that of the oak. It planes beautifully smooth, and has a pleasing appearance, on account of its dark streaks. The red kind, which grows very straight and spreads considerably, reaches a height of 50 feet, and from 12 to 15 inches diameter. The dark kind, which spreads still

more, seldom attains a height of 40 feet. Their name, axe-breaker, indicates their toughness; for in cutting or felling them, the axe is often broken. In cutting the samples received, two axes were nicked to the extent of one-half to three-fourths of an inch. The wood is durable and used for ties, poles, etc., and in posts for houses. One piece that had been for sixty-seven years in a clay soil was found still sound when sawed. The tree is common throughout the state.

(27.) *Quita-Calson.*—Its powder acts as a purgative. The tree attains a height of 30 or 35 feet, branching at 15 or 20 feet from the trunk, and often has a diameter of 2 feet. It is abundant along the coast. It is used for boards where not exposed to the weather; it will not resist moisture.

(28.) *Roble.*—Light colored wood of a curly and beautiful grain. It is pink in color and used in house-building. The tree has exceedingly large leaves, 14 inches long and about 7 inches wide, is often 50 feet high, and from 12 to 15 inches diameter. Is abundant along the coast.

(29.) *Ron-ron.*—This is one of the largest of hard-wood trees growing on the shores of the rivers of the Departmento of Chontales on the Atlantic slope. It reaches a height of 50 or 60 feet, and often a diameter of 3 feet. Dark, fine-grained wood, strong, heavy and durable. Is used in cabinet work, turns very easily, and is susceptible of polish. It turns dark with age.

(30.) *Tempisque.*—This tree is of historical interest in Nicaragua, and is one of the largest found in the tropical forests. It attains a height of seventy-five feet or more. The trunk is irregular and seldom reaches twenty feet to the beginning of the largest branches. It has a diameter of 6 feet. The wood is fine grained, hard and very excellent for desks and other articles of cabinet work. Like the mahogany, it turns dark in a few years, and is equally durable. The cattle eat its fruit.

(31.) *Tiguilote.*—Light wood, grows about 30 feet high, and over 12 inches in diameter; good wood for carriages. It is used for fence posts, it easily roots when set with care, thus making a permanent fence and a pretty grove.

(32.) *Zapotillo.*—Of a light mahogany color, has a fine grain, and is light. Attains a height of 40 feet and 1 foot in diameter. It is not very much used.

(33.) *Zopilote* (Buzzard).—Coarse, long-grained wood, but of compact layers when viewed in cross-section. It has a greenish color. Turns and planes fairly well. The wood is used only in neighborhoods where more useful woods are scarce. When unexposed or when well seasoned and protected with paint, it would probably be a valuable wood, as it ran above the average in torsion, was among the highest in compression, and stood well under transverse stress. It attains a height of about 40 feet and 1 foot in diameter.

The Transverse Stress tests were made in the Transverse testing machine of the Messrs. Fairbanks, designed for Prof. Thurston. It consists of a Fairbanks' scale combination with a pointer and beam at the end to secure perfect balance. The whole machine rests upon a wooden frame; upon the platform rests a cast iron cross beam, and upon this slide the supports, which are set and secured by bolts at the required distance apart The test piece is placed upon the mandrels, which rest on the supports set at the required distance. The loads are put in the scale, and equilibrium is established by the elastic resistance of the piece offered against the pressure transmitted through the screw and pressure block by means of the lever. The screw passes through a nut, and terminates in a sliding piece. The cast iron columns serve as guides to the pressure block. The whole is made stable by wrought iron braces bolted to the wooden frame. The deflections of the piece are measured by the linear advance of the screw, by means of a graduated wheel. The pitch of the screw was found to be equal to 0·33297 inches, and the wheel was graduated into 333 equal divisions, thus reading to $\frac{1}{1000}$ of an inch, with an error of 0 0001 of an inch. The pointer clamped to the screw is placed over the starting division in each case, and after the wheel has been turned and equilibrium established, the reading is taken. This is a very convenient way of reading the deflections, giving the difference directly. The test pieces were planed by hand by an expert, Mr. Kerr, and afterwards measured by means of a micrometer screw reading to $\frac{1}{1000}$ of an inch.

With all the data required, we obtained the results in the same way as in compression tests, by plotting the curve to each test, with loads as ordinates and the deflections as abscissas. This gives the elastic limit more exactly than in any other way. It also

shows at a glance the behavior, strength, elasticity, etc., of the material tested.

DETAILS OF TRANSVERSE STRESS TESTS.

(1.) *Carbon.*—28-inch supports. Green sample from the sap, and with a few small knots on top. Like the other, it broke just after having brought the beam up by its elastic resistance, under 650 pounds, and deflected 1·011 inches. Remained unbroken.

(2.) ——— ———.—25-inch supports. A very well seasoned piece, but slightly weakened by a knot where it first gave way. The first rupture occurred at 950 pounds on the weak part. The second under 1,150 pounds with a long and several other small splinters on tension side. Total deflection, 1·687 inches.

(3.) *Chaperno.*—24-inch. A very good sample as regards soundness and seasoning. Cross grained. Broke under 3,700 pounds, with a deflection of 0·496 inch. Broke gradually in small splinters from the bottom side upwards, and crushed slightly on top also.

(5.) *Cortez.*—24-inch. A large sample from near the heart. Excepting two knots on thickness and bottom sides respectively, and checked on sap line on top, it was a fine sample. Its toughness is shown by its behavior under load. Reached the elastic limit at 2,500 pounds, broke at 4,300 pounds, with a deflection of 0·443 inch. Second rupture occurred under 4,650, with a deflection of 1·3975 inch. By diminishing the loads it might have given more ruptures without breaking completely. Broke in several adhering splinters on tension side.

(7.) *Guachipilin.*—11-inch supports. Good sample. The first rupture occurred under 1,800 pounds load with 0·462 inch deflection. After the first rupture it showed a good deal of elasticity in resisting the loads and gradually ruptured slightly under 2,050 pounds. The reason we took this latter for the rupturing load is on account of the behavior after the first rupture, which latter was very light, and on account of a weak point. Both ruptures were very light splinters, and the piece might have shown still more tenacity if the load had been diminished. But here, as in most cases, we were after first rupture only. Almost any kind of wood will behave in the same way if so treated, unless it is very brittle.

(8.) *Guapinol.*—24-inch supports. An inferior sample, knotty near the middle, checked on top, and with a few worm holes. Broke under 1,600 pounds with a deflection of 0·699 inch and with a few splinters on tension side.

(9.) *Guayabo de Monte.*—24-inch supports. The test piece was planed from a green limb. It was checked and knotty on either side of middle line in tension side. The first rupture occurred on account of cross-grain at 660 pounds, with a deflection of 0·477 inch. Ruptured with only one splinter across the bottom side.

(11.) *Jenisero.*—24-inch. Broke at 1,600 pounds, with a deflection of 0·426 inches. When under 1,600 pounds load, brought the scale up when pulling the lever arm, but immediately afterwards broke suddenly in two splinters. It evidently is very brittle.

(18.) *Moran.*—15-inch supports. A very sound and beautiful natural-colored piece of wood. Ruptured in light splinters under 2,600 pounds, with a deflection of 0·432 inches. Remained unbroken and could have still shown tenacity in successive ruptures.

(20.) *Nancite.*—32-inch supports. A good sample. Broke square on tension side, in two pieces, and very suddenly, after having offered elastic resistance to 2,200 pounds, and deflected 1·0765 inches.

(23.) *Arco.*—24-inch. Straight-grained sample, with two knots on bottom side. Broke at 2,000 pounds, with a deflection of 0·679 inches. Second rupture occurred under 1,100 pounds load, with a deflection of 1·116 inches. Ruptured with close splinters on tension or bottom side.

(26.) *Quebra hacha negro.*—25-inch supports. Excepting a light streak of sap wood on bottom side, where it first gave away, it was a very sound sample. Its first rupture occurred when under 2,450 pounds, with a deflection of 1·693 inches. A second rupture occurred at 2,550 pounds, with a sudden crash and with a deflection of 2·080 inches. It is worth observing that the second rupture occurred with a larger stress than in the first one. The very finely intermingled narrow and regular splinters across the bottom side very fairly exhibit the high tenacity, ductility, as I may be allowed to say, and the uniformity of strength, cohesion and grain of this peculiar wood. Its rupture could very well be taken as the standard rupture of tough woods.

(27.) *Quita-Calson.*—21-inch supports. Season-checked on top,

TESTS BY TRANSVERSE STRESS.

	Wood.	Dist'nce between supports l	Depth h	Breadth b	Stress. E. Limit P	Stress. Rupture P'	Deflection at— E. Limit. d	Deflection at— Rupture. d'
		in.	in.					
1	Carbon, ...	28	1·235	1·470	300	650	0·392	1·011
2		25	1·225	1·215	500	1050	0·478	1·354
3	Chaperno, ...	24	2·000	1·994	2800	3700	0·36	0·496
4	Chiquirin, ...	24	1·772	1·779	1800	2400	0·420	0·611
5	Cortez, ...	24	1·740	1·745	2500	4300	0·443	1·073
7	Guachipilin, ...	12	1·237	1·237	1200	2050	0·277	0·761
8	Guapinol, ...	24	1·498	1·498	900	1600	0·335	0·699
9	Guayabo de Monte, . .	24	1·495	1·495	300	710	0·157	. .
11	Jenisero, ...	24	1·742	1·742	1000	1600	0·426	0·810
13	Laurel blanco, . .	28	1·775	1·730	1400	2150	0·139	0·265
14	Lligualtil, ...	30	1·218	1·218	300	750	0·552	3·284
18	Moran, ...	15	1·627	1·620	1600	2600	0·237	0·432
20	Nancite, ...	32	1·98	1·983	1200	2200	0·4385	1·076
22	Oja-tostada, ...	24	1·239	1·239	400	850	0·352	1·656
23	Palo de Arco, ...	24	1·459	1·492	1200	2000	0·382	0·679
26	Quiebra-hacha-negro, .	25	1·479	1·473	1200	2450	0·395	1·693
27	Quita-calson, ...	21	1·224	1·225	400	750	0·322	0·811
28	Roble, ...	30	1·772	1·768	1000	1600	0·615	1·217
30	Tempisque, ...	24	0·995	0·978	300	700	0·459	1·605
32	Zapotillo, ...	28	1·734	1·734	900	2100	0·318	1·139
33	Zopilote, ...	11	1·225	1·233	1450	2550	0·202	0·462
34	Guacuco, ...	30	1·48	1·480	500	1170	0·438	1·526
35	Escobillo, ...	30	1·745	1·741	0800	1850	0·216	0·826

	Wood.	Stress in outermost Fibre per Square Inch, at— E. Limit. $p = \frac{3\ Pl}{2\ bh^2}$	Stress in outermost Fibre per Square Inch, at— Rupture. $p' = \frac{3\ P'l}{2\ bh^2}$	Total Elastic Resilience $U = \frac{Pd}{2}$	Modulus of Elasticity. $E = \frac{Pl^3}{48\ d\ I}$
		lbs.			
1	Carbon, ...	5613	12162	58	1514400
2		10272	21571	119	1824400
3	Chaperno, ...	12575	16617	504	1672500
4	Chiquirin, ...	11600	15467	378	1496300
5	Cortez, ...	17025	29294	553	2119800
7	Guachipilin, ...	11411	19490	106	614550
8	Guapinol, ...	12206	21699	150	1858400
9	Guayabo de Monte, ...	3232	7649	23	1322000
11	Jenisero, ...	6803	6803	213	879780
18	Moran, ...	8385	13624	189	815100
20	Nancite, ...	7409	13583	263	1456500
22	Oja-tostada, ...	7571	16088	70	1665500
23	Palo de Arco, ...	13602	22670	229	2342900
26	Quiebra-hacha-negro, ...	13966	28514	237	2490200
27	Quita-calson, ...	6865	12872	64	1280300
30	Tempisque, ...	11154	26026	68	2344600
33	Zopilote, ...	12479	22730	141	1017100

and knotty on either side of middle line on the thickness side. Broke just under 750 pounds, with 0·811 inches deflection.

(33.) *Zopilote.*—11-inch supports. With a knot near one support gave away by shearing at the place when under 2,550 pounds. Stopped the test at this point and took this as the rupturing load.

The compression tests were made on the Olsen machine of 14,000 pounds capacity. The machine consists of four columns bolted to a plain cast-iron bed, and supporting on top a plate, in the middle of which one end of the test piece, if for tension, or the rod which holds the measuring apparatus are held by means of steel wedges.

Through the four angles of this bed and through those of that below pass four screws. To these latter is secured a plate, which pulls or compresses the piece, as the case may be. The plate rests upon four knife edges, on three beams or levers, and their ends rest together upon a link hung from knife-edges on each side of another beam, which latter is linked to the end of the scale in the same way. The machine is very sensitive. The scale is divided into divisions of five pounds. The load is applied by means of a crank or a lever, turning a central wheel geared to four others, one on each screw. A powerful leverage and a steady vertical motion of the plate is obtained. The deflections were measured by means of micrometer screws, using the electrical contact. I quote Prof. Thurston's full description of the instrument:

"The instrument consists essentially of two very accurately-made micrometer screws, working snugly in nuts secured in a frame which is fastened to the head of the specimen by a screw clamp. It is so shaped that the micrometer screws run parallel to and equidistant from the neck of the specimen on opposite sides. A similar frame is clamped to the lower head of the specimen, and from it project two insulated metallic points, each opposite one of the micrometer screws. Electric connection is made between the two insulated points and one pole of a voltaic cell, and also between the micrometer screws and the other pole. As soon as the micrometer screw is brought in contact with the opposite insulated point, a current is established, which fact is immediately revealed by the stroke of an electric bell placed in the circuit. The pitch of the screws is 0·02 of an inch, and their heads are divided into 200 equal

parts; hence a rotary advance of one division on the screw head produces a linear advance of one ten-thousandth (0·0001) of an inch.

"A vertical scale, divided into fiftieths of an inch, is fastened to the frame of the instrument, and set very close to each screw head and parallel to the axis of the screw; these serve to mark the starting point of the former, and also to indicate the number of revolutions made. By means of this double instrument, the extensions can be measured with great certainty and precision, and irregularities in the structure of the material, causing one side of the specimen to stretch more rapidly than the other, do not diminish the accuracy of the measurements, since half the sum of the extensions indicated by the two screws is always the true extension caused by the respective loads."

The size of test pieces advised for compression varies with different authorities, the limit of the ratio of the length to the diameter being five times the diameter. The compression test should crush the piece down or shear it at 45°, but even with the ratio used—the length equal to twice the diameter, as given and used by Prof. Thurston—the piece showed a slight buckling in some cases.

We have obtained the results, from each test by plotting a curve, taking the loads as ordinates and the deflections as abscissas. In this way we see the behavior of the piece at a glance, by the curve, and find the elastic limit very accurately. This latter was obtained by drawing a straight initial line when necessary, and taking it as the point where the curve leaves the line.

We have given not only the results derived from the most common formula used in designing, or used indirectly, but also the original data of size of test piece, the actual loads, unit strains and stresses as they may be found convenient in some cases and for further information if desired. The formulæ from which the results were derived are set at the head of the tables.

TESTS BY COMPRESSION.

	Name.	Original Length.	Original Diameter.	Final Length.	Stress. E. Lim.	Stress. Maxim.	Stress. Break'g	Strain. E. Limit.	Strain. Maxim.	Strain. Breaking.
2	Cedar,	1·656	0·844	1·6273	2100	2300	2300	0·0189	0·0289	0·0289
5	Cortez,	1·625	0·828	1·5815	5500	8100	8100	0·0224	0·0435	0·0435
6	Granadillo-negro,	1·552	0·729	1·4733	4000	6500	6070	0·0115	0·0307	0·0795
7	Guachipilin,	1·583	0·732	1·5546	3000	5000	5000	0·0102	0·0289	0·0289
9	Guayabo de Monte,	2·235	1·125	2·1893	6000	8500	8500	0·0191	0·0460	0·0460
11	Jenisero,	1·506	0·794	1·4632	3000	4000	4050	0·0179	0·0341	0·0432
14	Lligualtil,	1·609	0·828	1·5157	4200	6325	6325	0·0178	0·0804	0·0936
15	Madera-negra,	1·609	0·812	1·5416	5500	6700	6650	0·0326	0·0574	0·0677
16	Madrono,	1·625	0·812	1·5638	4800	6600	6000	0·0156	0·0335	0·0612
21	Nispero,	1·625	0·828	1·5760	3500	7000	7000	0·0142	0·0490	0·0490
22	Oja-tostada,	1·218	1·0859	2·1573	5000	8000	7800	0·0130	0·0316	0·0613
26	Quiebra-hacha,	1·625	0·8281	1·5776	5400	8600	8375	0·0225	0·08[illegible]4	0·0474
27	Quita-calson,	2·224	1·119	2·1743	5000	8000	7600	0·0130	0·0318	0·0495
29	Ron-ron,	1·625	0·828	1·1823	4200	5900	5900	0·0165	0·0427	0·0427
31	Tiguilote,	2·219	1·126	1·1941	3800	5000	5000	0·0164	0·0251	0·0251
36	Guanacaste,	1·969	0·817	1·5560	3000	3700	3700	0·0135	0·0409	0·0409

	Wood.	Unit Stress. E. Limit. $p = \frac{P}{F}$	Unit Stress. Maxim. $p' = \frac{P'}{F'}$	Unit Stress. Breaking. $p'' = \frac{P''}{F''}$	Unit Strain Elastic Limit. $\epsilon = \frac{\lambda}{L}$	Modulus of Elasticity. $e = \frac{p}{\epsilon}$	Modulus of Resilience $U = \frac{p\,\lambda}{2\,L}$
1	Carbon,	5600	9800	9585	0·0069	804860	19
3	Chaperno,	2100	11500	11500	0·0035	593540	3
8	Guapinol,	7800	12000	12000	0·0074	1051600	28
10	Guiliguiste,	6000	8800	8800	0·0067	882940	20
12	Jicaro-sacaguacal,	4000	7800	7800	0·0096	414020	19
20	Nancite,	6000	8000	8000	0·0072	824340	21
21	Nispero,	5600	10275	10000	0·0077	723550	21
21	Nispero,	7000	14000	14000	0·0087	801050	30
33	Zopilote,	7200	10800	10500	0·0099	721370	35

	Wood.	Unit Stress.			Unit Strain Elastic Limit.	Modulus of Elasticity.	Modulus of Resilience
		E. Limit. $p = \frac{P}{F}$	Maxim. $p' = \frac{P'}{F}$	Breaking $p' = \frac{P''}{F}$	$E = \frac{\lambda}{L}$	$E = \frac{p}{\epsilon}$	$U = \frac{p\lambda}{2L}$
2	Cedro, ...	4200	4600	4600	0·0114	367190	18
5	Cortez, ...	11000	16200	16200	0·0137	797990	75
6	Granadillo-negro, .	8000	13000	12140	0·0074	1080300	29
7	Guachipilin, ...	6000	10000	10000	0·0064	931470	19
9	Guayabo de Monte, .	6000	8500	8500	0·0085	702180	25
11	Jenisero, ...	6000	8000	8100	0·0118	504880	35
14	Lligualtil, ...	8400	12700	12650	0·0110	759440	46
15	Madera-negra, . . .	11000	13400	13300	0·0202	543010	111
16	Madrono, ...	9600	13200	12000	0·0096	1000000	46
21	Nispero, ...	7000	14000	14000	0·0087	801050	30
22	Oja-tostada, ...	5000	8000	7800	0·0058	853320	14
26	Quiebra-hacha, . .	10800	17200	16740	0·013846	780000	74
27	Quita-calson, . .	5000	8000	7600	0·0058	855310	14
29	Ron-ron, . . .	8400	11800	11800	0·0101	827270	42
31	Tiguilote, ...	3800	10000	10000	0·0073	514210	14
36	Guanacaste, . . .	6000	7400	7400	0·0084	709730	25

	Wood.	Original.			Stress.			
		Length. L	Diameter D	Final Length. L	E. Limit. P	Maxim. P'	Breaking. P''	Strain E. Limit. λ
1	Carbon, . . .	2·2421	1·128	2·1562	5600	9800	9585	0·0156
3	Chaperno, . .	2·2611	1·1245	2·2096	2100	11500	11500	0·0080
8	Guapinol, . .	2·2516	1·3115	2·2036	7800	12000	12000	0·0167
10	Guiliguiste, .	2·2515	1·128	2·2012	6000	8800	8800	0·0153
12	Jicaro-sacagua-cal, ...	1·625	0·8437	1·5534	2000	3900	3900	0·0157
20	Nancite, . . .	2·2532	1·1292	2·2238	6000	8000	8000	0·0164
21	Nispero, . . .	2·2611	1·1235	1·9522	5600	10275	10000	0·0175
21	Nispero, . . .	1·625	0·8281	1·5760	3500	7000	7000	0·0142
33	Zopilote, . . .	2·2704	1·126	2·2143	7200	10800	10500	0·0180

The torsion tests were made in the autographic testing machine of Prof. Thurston: It consists of two angle frames united at their vertices by a cast-iron rod and rigidly bolted to a heavy cast-iron bed, thus making the machine very firm. These two angles have the bearings in line with the jaws or wrenches which hold the piece to be tested by means of steel wedges put from opposite sides. The test piece is put in line before securing it rigidly between the jaws, by means of centres in each jaw, one resting on a spiral spring and the other turned by a screw which projects out of the frame, thus enabling to place the piece symmetrically and directly in line with the axis of rotation.

The outer end of one of the jaws is connected to a worm-wheel, the rotation to which is imparted through the worm by means of the crank. The other jaw carries the pendulum, to which is connected, a little below the jaw, the pencil which is held by a spring, tight against the sides of the guide curve. This curve is made so that when the pencil rolls on it, in virtue of the swinging of the pendulum through the test piece, the ordinates which are recorded upon a cross-section paper carried by a drum on the opposite jaw or on the one connected to the worm wheel, are proportional to the moments about the axis of the test piece. The machine, by the tracing of a simple curve, tells all the characteristics of the test piece. The circumference of the drum is equal to thirty-six inches, and the inches in the paper are divided into ten equal parts, thus making each inch of paper equal to 10°. The vertical lines are also divided in inches and each into tenths.

When the piece is secured between the clamps, the crank is turned by hand with a uniform motion and a slow rotation, interrupted only by the rupturing of the piece, is given to it, causing the pendulum to swing up on one side. This measures the resistance to torsion of the test piece which is recorded autographically in the paper.

To make these tests, we took off the bob from the pendulum, as it offers too large a moment for the torsional resistance of the woods for the size of test piece used. Before making the tests, we found the necessary constants of the machine to work out the results of tests, as follows: The pendulum without the bob was supported horizontally by a column, which rested on a scale. Thus, the maximum moment of the pendulum was weighed, and the corresponding ordinate in the curve observed:

Lever arm,	48 inches.
Weight of column with lever arm,	52·75 pounds.
" supporting columns	21·5 "
" lever arm, or pendulum,	31·25 "
Hence maximum moment,	1,500 inch-pounds.

Maximum ordinate traced by the pencil, or ordinate corresponding to horizontal position of the pendulum — 4·3 inches. Therefore, one inch of ordinate = 348·837 inch-pounds.

We found the friction of the machine by means of a delicate spring balance, attached at a distance of fifty inches from the

centre of suspension. The pendulum was pulled just far enough to overcome friction alone. After several trials, it was found to be equal to 0·25 pounds. Thus, the friction of the machine is 12·5 inch-pounds, which friction is added, in every case, to the moments recorded by the pencil.

The absolute errors in the size of the test pieces vary by excess from above one-thousandth ($\frac{1}{1000}$) of an inch up to a little above one-hundredth (0·01) of an inch.

We have put down the moments at the elastic limit and at the maximum before rupture; also the angles of torsion at the elastic limit, maximum before rupture, and final rupture with the corresponding elongations of the outermost fibres for the first three angles. As a matter of detail, we have observed those which reached the final rupture with a higher moment after the first rupture, and those that did not offer a higher resistance. The names used express the character of the four critical points to which a material is subjected under an increasing stress.

Not being able to put in the strain diagram, we have endeavored to write out for illustration the behavior of several pieces under stress, and their mode of rupture as concisely as possible in the details of torsion tests. These may be more valuable than the numerical results. The torsion tests give a better and more general idea of all the properties of strength and elasticity of the woods than any other test, as, in this case, they are compelled to write for themselves their "own story."

DETAILS OF TORSION TESTS.

(1.) *Carbon.* d = 0·6271.—Test piece turned from a large sample near the heart. Very sound in the turned part. Elastic limit, maximum and rupture occurred under the same moment. From this and by breaking in two splinters it shows it is a brittle wood. It reached a moment at the latter points of 200·87 inch-pounds, at an angle of 7°·5 and gave away completely at 13°·2 with no higher moment. Its curve was at 45° from the axis of angles and was short-wavy all along.

(2.) ————.—With a short wavy and parabolic curve reached its elastic limit at 7°·11 under a moment of 186 inch-pounds, and reached a maximum of 190 inch-pounds moment, which kept constant for about 8°, when it came down and ruptured. Fairly regular stiff rupture.

(3.) *Chaperno.* $d = 0{\cdot}6278$.—Turned from a large sample and very sound piece. It rose with a 45° wavy curve up to 104·9 inch-pounds when it reached the elastic limit. Went up to a maximum of 124·128 inch-pounds, which kept constant until final rupture, splintered at 17°·3.

(5.) *Cortez.* $d = 0{\cdot}6278$.—Turned from a large sample near the heart. Very sound piece. It rose with a 45° curve and slightly wavy but uniform, reaching the elastic limit at 214·82 inch-pounds at an angle of 5°·9. Ruptured at 28° and splintered at 41°, reaching afterwards a higher moment, which kept constant till final rupture. Fibrous and stiff rupture, shearing a plug of same cross section as the turned part from the shoulder.

(8.) *Guapinol.* $d = 0{\cdot}5645$.—With a slightly parabolic curve at 45° angle, reached its elastic limit at 6° with a 183 inch-pounds moment, which kept constant until first rupture and reached a little higher moment before final rupture. Gave away with wide fibrous splinters.

(12.) *Jicaro-sacaguacal.*—The test piece was turned from a green limb. It rose with a parabolic line and very much inclined toward the axis of strains reaching at the elastic limit a moment of 103·2 inch-pounds. Reached a higher moment, which kept constant until final rupture at 155°, when it completely broke irregular and stiff.

(13.) *Laurel.* $d = 0{\cdot}631$.—Dark color. With a low sloping curve, but uniform, reached the elastic limit at 4° angle and under a moment of 82·268 inch-pounds. This was its maximum resistance and kept constant until ruptured at 20°. Uniform square rupture around the shoulders without projecting splinters. Gave way entirely at 105° angle.

(17.) *Mahogany.* $d = 0{\cdot}6358$.—A very sound piece, turned from a large sample. It rose with a long wavy line up to 78·779 inch-pounds, its elastic limit at 3° angle. Reached a higher moment of 82· inch-pounds at 6°·7 when it ruptured. It splintered at a still little larger moment at 27°·6. Fibres wide and stiff, and rupture irregular.

(18.) *Moran.* $d = 0{\cdot}6256$.—A very sound piece. With a curve above 45° angle with the axis of strains reached its elastic limit at 2° under a moment of 82·27 inch-pounds, which kept constant for 1·5, when it rose to 88·95 inch-pounds moment, and gave away for

the first time. However, it still reached about twice as large a moment which kept constant till final rupture. Fibrous and tough rupture.

(24.) *Piedra* (Stone). $d = 0{\cdot}6257$.—A piece turned from near the heart and very sound. It rose with a parabolic short wavy line up to the elastic limit, when it had a moment of 227 inch-pounds at an angle of $9^\circ{\cdot}7$ It reached a maximum moment of 228 7 inch-pounds at an angle of 11°, and very soon lowered its moment to 221·8 inch-pounds, which kept constant until it splintered at 50°. Wide splinters and stiff rupture, but square around the shoulders.

(26.) *Quiebra-hacha.* $d = 0{\cdot}6268$.—A very sound piece. It rose with a parabolic curve above 45°, with the axis of strains up to 166 inch-pounds moment, its elastic limit, and kept reaching higher and higher moments by regular intervals up to a maximum after first rupture of 319·479 inch-pounds without giving way completely. Brushy, regular break and very tough.

(29.) *Ron-ron*.—Dry and sound piece. Rose with a straight line up to 82·26 inch-pounds moment at an angle of $1^\circ{\cdot}5$, and kept a constant moment till final rupture. Broke with brittle fracture in two pieces, without showing any fibre.

(32.) *Zapotillo.*—The test-piece turned from a large sample, $d = 0{\cdot}6258$ inch; had a small knot on one end, but very sound otherwise. It reached its maximum at the elastic limit at an angle of $5^\circ{\cdot}5$ from the origin, rupturing with the same moment at angle of 9°. It kept a constant moment for about 80°, when it reached a little higher moment holding it till final rupture. Fibrous rupture.

(33.) *Zopilote.*—At first rose with a straight line below 45°, and turned parabolic near and after the elastic limit. First ruptured at 28° angle under a 106 inch-pounds moment and raised it slightly, keeping the latter constant for over 160. A very thready and tough rupture.

TESTS BY TORSION.

No.	Wood.	Moments of Torsion in in.-lbs. E. Limit $(P_a)_E$	Max. before Rupt. $(P_a)_M$	Angle of Torsion in Degrees. E. Lim a	Max. before Rupt. a_1	First Rupt. a_2	Maximum Shearing Stress. Elastic Limit. $p^3 = \frac{2}{\pi r^3} \times (P_a)_E$	Maximum before Rupture. $p_s = \frac{2}{\pi r^3}(P_a)_M$
2	Cedar, . .	43·89	45·63	3·6	8·4	8·4	915	952
5	Cortez, . .	68·3	71·80	3·3	19·	27·5	1424	1497
7	Guachipilin,	82·2 8	148·54	2·6	49·	49·	1716	3098
9	Guayabo de Monte, .	54·36	64·82	4·5	5·5	26·5	1133	1352
13	Laurel-macho, .	68·31	71·8	2·8	4·1	4·1	1424	1497
15	Madera-negra, .	141·57	141·57	3·6	13·4	17·4	2953	2953
16	Madrono, .	99·71	103·19	4·5	29·	35·0	2080	2152
19	Nacascolo,	145·06	145·06	2·4	2·4	16·1	3026	3026
20	Nancite, .	61·34	68·3	2·7	12·	12·	1279	1424
21	Nispero, .	103·89	193·89	7·2	5·	9·5	4044	4044
22	Oja-tostada,	52·616	54·36	2·7	14·5	19·5	1097	1133
25	Pochote, .	47·38	50·87	5·0	5·8	5·8	988	1061
26	Quiebra-hacha (yellow),	152·03	152·03	5·0	5·	9·3	3171	3171
28	Roble, . .	54·36	54·36	3·3	3·3	12·	1133	1133
31	Tiguilote, .	33·43	43·87	5·4	17·5	17·5	697	915
36	Guanacaste,	68·31	71·8	2·25	3·2	3·2	1424	1497

No.	Wood.	Moments of Torsion in in.-lbs. E. Limit $(P_a)_E$	Max. before Rupture $(P_a)_M$	Angle of Torsion in Degrees. E. Limit a	Max. before Rupture a_1	First Rupture a_2	Maximum Shearing Stress. Elastic Limit. $p_s = \frac{2}{\pi r^3}(P_a)_E$	Maximum before Rupture. $p_s = \pi r^3 (P_a)_M$
1	Carbon, . .	200·8	200·8	7·5	7·5	7·5	4190	4190
2		186·92	190·4	7·11	8·5	8·5	3899	3972
3	Chaperno, .	104·9	124·1	3·4	5·7	7·3	2189	2589
5	Cortez, . .	214·8	214·8	5·9	5·9	28·0	4481	4481
7	Mahogany,	78·7	82·2	3·0	6·7	6·7	1643	1716
8	Guapinol, .	183·4	183·4	6·0	18·2	18·2	3826	3826
12	Jicaro-saca-guacal, .	103·2	110·1	20·	22·0	22·0	2153	2298
13	Laurel, dark,	82·2	82·2	4·0	4·0	20·0	1716	1716
18	Moran, . .	82·2	88·9	2·0	4·2	4·2	1716	1855
24	Piedra, . .	227·0	228·7	9·7	11·0	11·	4736	4772
26	Quiebra-hacha, . .	166·	166·0	8·0	8·0	8·0	3463	3463
29	Ron-ron, .	82·2	82·2	1·5	23·5	23·5	1716	1716
32	Zapotillo, .	99·7	99·7	5·5	5·5	9·0	2080	2080
33	Zopilote, .	94·4	106·6	5·7	28·0	28·0	1971	2225

	Wood.	Extension of outer Fibre. E. Limit. $\lambda = \sqrt{1 + a^2 (0\cdot0000029747)} - 1$	Max. before Rupture.	First Rupture.	Modulus of Elasticity. $E_s = \frac{(P_a)E}{a} \times \frac{l}{I_p}$	Total Elastic Resistance. $\frac{(P_a)E \times a}{2}$
2	Cedar, . . .	. . .	. . .	. . .	46635	1
5	Cortez, . . .	. . .	. . .	. . .	79161	2
7	Guachipilin, .	. . .	. . .	. . .	152358	2
9	Guayabo de Monte, . .	. . .	. . .	. . .	46203	2
13	Laurel-macho,	. . .	. . .	. . .	93310	1
15	Madera-negra,	. . .	. . .	. . .	150407	4
16	Madrono, . .	. . .	. . .	. . .	84748	4
19	Nacascolo, .	. . .	. . .	. . .	231175	3
20	Nancite, . .	. . .	. . .	. . .	86893	1
21	Nispero, . .	. . .	. . .	. . .	102997	12
22	Oja-tostada, .	. . .	. . .	. . .	74533	1
25	Pochote, . .	. . .	. . .	. . .	36243	2
26	Quiebra-hacha (yellow), .	. . .	. . .	. . .	116297	6
28	Roble, . . .	. . .	. . .	. . .	63003	1
31	Tiguilote, . .	. . .	. . .	. . .	23678	1
36	Guanacaste, .	. . .	. . .	. . .	116119	1

	Wood.	Extension of outer Fibre. E. Limit. $\lambda = \sqrt{1 + a^2 (0\cdot0000029747)} - 1$	Max. before Rupture.	First Rupture.	Modulus of Elasticity. $E_s = \frac{(P_a)E}{a} \times \frac{l}{I_p}$	Total Elastic Resistance. $a = \frac{(P_a)E \times a}{2}$
1	Carbon, . . .	. . .	. . .	. . .	102440	13
2		. . .	. . .	. . .	100551	11
3	Chaperno, . .	. . .	. . .	. . .	118050	3
5	Cortez, . . .	. . .	. . .	. . .	139270	11
8	Guapinol, . .	. . .	. . .	. . .	116920	9
12	Jicaro-sacaguacal, . .	. . .	. . .	. . .	19735	18
13	Laurel (dark),	. . .	. . .	. . .	78663	2
17	Mahogany, .	. . .	. . .	. . .	100440	2
18	Moran, . . .	.	. . .	. . .	157330	1
24	Piedra, . . .	. . .	. . .	. . .	89522	19
26	Quiebra hacha,	. . .	. . .	. . .	79363	11
29	Ron-ron, . .	. . .	. . .	. . .	209750	1
32	Zapotillo, . .	. . .	. . .	. . .	69339	04
33	Zopilote, . .	. . .	. . .	. . .	63390	4

BOOK NOTICES.

LETTERS-PATENT FOR INVENTIONS. By J. McC. Perkins. Boston, Mass.: Rand, Avery & Co.

This little pamphlet merits the highest commendation, as it sets forth the truth upon a matter of the greatest importance. In this we speak advisedly, for it is admitted by the foremost minds in sociology that the protection and encouragement of invention is one of the most important factors in national supremacy.

After reviewing the law on the subject, the author makes the following candid statement of a fact that it is of the utmost importance that inventors should fully understand. The passage is: "It is therefore evident, that the inventor, by proper management, can keep alive his right to a patent for an indefinite period, and this notwithstanding the invention may have been on sale or in public use for over two years before the grant of the patent."

Attention is directed to the importance of having the claims properly drawn, so as to avoid the necessity for a re-issue, with its expense and unsatisfactory results. The law of trade-marks and labels is also reviewed.

Every inventor should read this work, as a knowledge of its contents is of the most practical value, and will enable the applicant for a patent to avail himself of a certain feature of patent law of which many are ignorant.

W. B. C.

WINDING MAGNETS FOR DYNAMOS. By Carl Hering. London and New York: E. & F. N. Spon. pp. 63.

The above excellent little book is a revised and enlarged reprint of a series of articles that first appeared in the *Electrical World*, of New York.

In designing a dynamo, which at a given speed shall produce a certain definite potential and current, a difficulty arises in practice in calculating the exact proportions of certain parts with the necessary degree of accuracy. This arises, to a great extent, from the variable values on which many of these proportions depend. Chief among some of these troublesome variables may be mentioned the following, viz.: The effect on the magnetic field of the varying magnetic qualities of the iron used; the shape and relative size of the pole pieces; the cross section and length of the magnet cores; the magnetic resistance of the revolving armature; the combined effect of the magnetism of the pole pieces and that of the revolving armature on the diameter of commutation; the effect of magnetic leakage; the variations in the resistance of the field magnet coils or the armature coils by self-induction; the varying effects of eddy currents in the pole pieces and armature core, or in the copper of the armature when sufficiently heavy; deleterious induction produced by sudden changes in the magnetization following sudden fluctuations in the current, etc. Though calculations for the quantitative action and interaction of these influences on the electro-motive force and current are possible in some cases, yet

the results obtained cannot be entirely relied on, and the author has therefore adopted the plan of completing the machine except the winding of the field magnets, and then making a practical test for the purpose of finding out the actual number of ampère-windings required.

This plan, of course, eliminates the above series of errors, and replaces them by a sure and practical method.

We note as new among the formulæ employed by the author, those for the direct calculation of the least diameter of the wire on the field magnets, having given the cores of the magnets, and the coil space, and having found by experiment the number of ampère-windings.

It will be noticed that the plan proposed supposes the machine completed, with the exception of the windings of the field magnets; the speed has been determined, neither too low for economy nor too high for safety; the lead of the brushes is such as to obtain quiet running; the armature details are also determined, and hence also the current the machine is able to furnish.

With the above conditions, there remains, therefore, the determination of the details of the field, under which such an armature, running under the pre-determined conditions, will develop the electro-motive force necessary to produce the pre-determined current that is to flow through it.

The speed, the armature details, and the dimensions of the frame being given, there remains but one factor to produce the required electro-motive force, viz., The magnetism of the field magnets, and this the author determines by running the machine at its calculated speed, and placing on its field magnets temporary coils the exact number of windings in which is known, and then exciting the field by some external source, until the current produced by the armature with a quiet commutator is equal to what it was designed to produce. When the machine has reached as high a temperature as it will acquire during actual use, the exciting current is measured, when the product of the exciting current in ampères and the number of turns in the temporary coils will give the number of ampère-windings. The formulæ before referred to are those employed for the calculation of the correct size of the wire, and the number of its turns to produce the requisite intensity of the magnetic field.

The author shows how the calculation of the ampère-windings may be used to show whether or not the dynamo is properly proportioned, and if not, where the fault lies.

It is but fair to the author to state that the book treats of the determination of the winding of the field coils only, as its title clearly shows, and not, as a recent reviewer assumed, of the building of the entire dynamo.

The author discourses the application of the preceding principle to the case of series, shunt, and compound-wound machines. E. J. H.

SCIENTIFIC NOTES AND COMMENTS.

ASTRONOMY.

THE ALMUCANTAR.—Prof. S. C. Chandler, Jr., in *The Annals of the Astronomical Observatory of Harvard College*, **17**, publishes an extended investigation of the character of work to which his new astronomical instrument, the almucantar, may be applied. The present memoir is in development of the plan already indicated at the Boston meeting of the American Association of Science, 1880, and will probably have an important bearing upon that class of observations which must ever remain the basis of practical astronomy. It is well known that the transit circle has, during many years, been exclusively used for determining the fundamental positions of stars. The transit, however, requires delicate and tedious manipulation, is subject to errors requiring lengthy investigation, and is withal expensive. Moreover, in the ordinary course of transit work, systematic errors may enter, and from lack of a check method, entirely escape detection. It is, therefore, with no indifferent interest that astronomers will scan the results obtained in the same field by a cheaper instrument of novel form, and subject also to novel theoretical and manipulative conditions.

Described in the briefest terms: The almucantar is a floating altazimuth, in every azimuth quickly adjusting itself to the horizontal circles to which it is directed. An upright cylindrical pillar, branching at the base into a tripod, with levelling screws, is surmounted by an iron trough 31 inches long, 6 inches broad and 1 inch deep. This trough, by means of a conical sleeve upon which it rests, may be moved in azimuth and the position of the instrument noted on a horizontal finding circle, concentric with and attached to the pillar. A suitable float rests upon the mercury of the iron trough, and to the float are attached the bearings upon which the horizontal axis of the telescope rotates. An altitude circle is attached to this axis. The telescope has, in the given instrument, an aperture of about 4 inches and focal length of 44 inches, and is provided with a ruled Rogers reticule and suitable field illumination. The angle between the telescope and float remaining constant, the conditions of equilibrium also continue precisely the same, in whatever azimuthal direction the instrument may be pointed. Hence, with the telescope clamped at any given altitude, "the sight line will mark accurately in the heavens a horizontal circle, and the transit of stars, as they rise or fall over this circle in different azimuths, will furnish the means of determining instrumental and clock corrections, the latitude, or right ascensions and declinations." The horizontal circle or almucantar, which passes through the pole, is the one preferably adopted as the plane of reference, just as the vertical circle passing through the pole, the meridian is adopted in the case of the transit instrument. Prof. Chandler gives the mathematical theory of the instrument in detail, and illustrates the formulæ by numerical examples. In an extensive chapter on the results of observations, are given: The results of latitude determinations of Harvard College Observatory by means of the

almucantar, showing remarkable accord with the zenith telescope determination of Dr. Gould, and exhibiting also a practically identical result for two almucantars of very different aperture; also, comparative time determinations with the meridian circle and almucantar by Chandler and Rogers, showing the reliability of the latter instrument, and leading the author to one of the most comprehensive discussions on personal equation anywhere to be found.

Another chapter is devoted to results of experiments instituted for the purpose of testing the general efficiency of the instrument and method. With respect to the important point of the precision with which the floating instrument regains a normal position of equilibrium, Prof. Chandler proves the probable error of equilibrium to lie inside of one-tenth of a second of arc, or the range of variation so minute as not to be certainly measurable. The memoir closes with a comparison of almucantar right ascensions with other recent observations; with a number of ingenious suggestions as to forms and uses for the instrument, and with a table giving the particulars of observations previously discussed. The volume contains all the materials for a just estimate of the instrument, and will sooner or later doubtless lead to the use of the new method in connection with more than one important problem in practical astronomy.

M. B. S.

The Yale Heliometer and its Work.—William L. Elkin, in the *Transactions of the Astronomical Observatory of the Yale University*, vol. i, part 1, recently issued, presents, along with a brief description of the characteristic features of the new heliometer, the extensive and valuable researches made by him on the Pleiades group. The heliometer is from the hands of the Messrs. Repsold, and pronounced of the highest order of excellence. The aperture of the object glass is 151 mm., and the approximate focal length 2,495 mm. The instrument has, by the advice of Mr. Elkin, been furnished with a recording micrometer, and also been much improved in other important details.

The first piece of work planned and carried to successful completion by Mr. Elkin, was nothing less than a complete redetermination of the relative places of the principal stars forming the group of the Pleiades. This problem was one that had occupied the attention of the celebrated Bessel at Königsberg, during some twelve years, his work being completed in 1841. Apart, therefore, from the advantages this line of research presented in testing the new instrument, the redetermination of the places was in itself desirable as affording further knowledge of the group and its motions.

Mr. Elkin has employed two independent methods for determining the relative position of the stars of the group, the one based on the measurement of distances, the other of position, angles and distances. For the former method four stars in the outer limits of the group and forming a quadrilateral, were selected as standard stars, and their positions deduced from carefully determined meridian observations, made at two European observatories. The angular distance of each star from each of the four reference points was measured and thence the corrections to the assumed places computed. There was a high presumption in favor of this method, on account of the superior accuracy of distance measures, and this the result verifies. The second

method employed was that used in the triangulation by Bessel; namely, measurement of distance and position-angle from Alcyone, the central star of the group. Both independent sets of measures are then combined by a refined discussion of errors and the resulting right ascensions and declinations of the sixty-nine observed stars tabulated, with the precessions and secular variations computed from the Pulkova constants. These results, reduced to an epoch forty-five years later than Bessel's, furnish material naturally suggestive of a careful comparison, which Mr. Elkin undertakes. First, however, he finds it necessary to subject the Königsberg results to a new reduction, and the revised measures are then compared with the Yale results. Of the fifty-one stars compared, Mr. Elkin makes the deduction that "there are thirty-two for which there is some considerable probability of relative displacement since 1840." A striking result is that showing the six largest displacements to have a remarkable community of direction, which is also approximately equal in amount and reverse in direction to the absolute motion of Alcyone for the same period. These six stars are, therefore, probaby not strictly members of the group, but only optically so. The internal motion of the group appear to be extremely minute and "the hopes of obtaining any clew to the internal mechanism of this cluster seem therefore not likely to be realized in an immediate future." Yet it would be impossible to express too high an estimate of the scientific value of work of the kind just completed at Yale by Dr. Elkin, since it forms a reliable basis for most important future research.

M. B. S.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, September 21, 1887.*]

HALL OF THE INSTITUTE, September 21, 1887.

MR. JOSEPH M. WILSON, President, in the Chair.

Present, 110 members and thirteen visitors.

Additions to membership since last report, thirty-one.

MR. WM. M. HENDERSON, of Philadelphia, read the paper for the evening, on "An Improved Triple-Expansion Engine." The paper, with discussion thereon, has been referred for publication.

Prof. ARTHUR BEARDSLEY, of Swarthmore College, exhibited a number of specimens of textile fabrics, paper, wood, etc., which had been rendered non-inflammable by the "Martin Fire-proofing Process," and gave an oral account of the advantages of the process. Dr. HENRY A. MOTT, of New York, by request, explained what substances were employed in the process, and how they were used; and gave an account of the tests to which the process had been subjected in France, and in the United States. An abstract of the proceedings on this subject, has been prepared for publication. The Secretary presented his usual monthly report.

Adjourned.

WM. H. WAHL, *Secretary.*

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[*Contribution from the Dept. of Civil Engineering, University of Penna.*]

IMPROVEMENT OF THE PORT OF PHILADELPHIA.

BY PROF. L. M. HAUPT, C.E.

Navigable water is essential to the existence of a commercial city, and it was largely due to the facilities for effecting a landing that this site was selected by the founders of Philadelphia.

But in the two centuries that have passed, the city has outgrown the limits of its deep water frontage, and although its shore line bordering a navigable river, has an extent of nearly twenty-five miles from the Poquessing Creek to Fairmount, but a limited portion of it is available without incurring great expense.*

* The importance of this discussion is confirmed by the accompanying extract found in the Philadelphia *Record*, September 23, 1887, a few days after this paper was completed. L. M. H.

A FOREST OF SPARS.—VESSELS IN PORT THAT CANNOT BE UNLOADED. —*The City's Growing Commerce Retarded by Inadequate Wharfage.—Corporations Monopolizing the River Front.*—Within the last few years the

The deepest water is to be found in the six mile concavity extending from Greenwich Point to just above Port Richmond Of the remainder about eight miles are cut off by the Five Mile Bar, stretching from the head of Petty's Island to Point No Point; seven miles are included in the Schuylkill River, which is only about 400 feet wide, and the rest is covered by the Horse-shoe Shoals.

Above Five Mile Point, there are twenty-six feet of water very nearly to Torresdale, with the one exception of a cross-over bar opposite the House of Correction, having on it over eighteen feet. Moreover, sixteen feet of water can be carried at mean low tide almost to the mouth of the Delaware and Raritan Canal, by removing the one short confluent bar at Kinkora, thus greatly augmenting the carrying trade by river and reducing the cost of transportation. The commerce of the upper river to and from Bordentown for the year ending December 31, 1885, is estimated to have been 1,135,942 tons, independently of the freight and passengers carried by the daily steamers from Philadelphia to points above.

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commerce at the port of Philadelphia has been on a steady increase despite the inadequate facilities at hand for the reception of traffic, until to-day the sorry sight is presented of a harbor teeming with heavily-laden vessels that must wait for days before wharfage can be procured for the discharge of their cargoes.

At the present time there is more shipping anchored in the Delaware River between Port Richmond and the League Island Navy Yard than ever before in the history of the port. Looking southward from Dickinson Street yesterday, the eye encountered a great number of idle vessels, whose spars, looming up toward the sky, resembled a vast forest. In this great fleet of the merchant marine all the nations of the world were represented and every description of cargo included, from East India jute to the common article of commerce—Spanish iron ore. The detention of this vast amount of traffic at the entrance to the second city of the Union is due solely to the lack of available places at which to discharge it. The scarcity of wharf property arises mainly from the fact that nearly every foot of wharfage from Clearfield Street to Moore Street is either under corporate ownership or control, or has been rendered valueless through the refusal of the railroad companies to introduce the necessary sidings and connections that would prevent the necessity of handling goods a second time.

In many cases wharf charges are maintained at such an exorbitant rate that vessels have been driven away by the excessive tolls. Another impediment to the natural growth of the port is the contracted dimensions of many of the docks, which are too small to admit of the docking of vessels, so that they will not project into the river to the danger of passing craft. In other

Concerning that part of the Delaware within city limits, and lying above Petty's Island, the Advisory Board to the Harbor Commissioners, of which Capt. G. B. White, is Chairman, says,* "It is almost in a state of nature, but has great qualifications for commercial frontage. The main channel lies in great bends, but these are favorable to bold frontage. Were the river straight, the channel would be midway between the two shores, and wharves of at least 1,000 feet in length would be necessary to reach the border of the ship channel." * * * "In short, the natural advantages of this whole frontage above Bridesburg are unusual." He adds, "The city front above Richmond has been but little utilized, because of the great shoal that has accumulated in the neighborhood of Five Mile Point, and which the General Government now designs to remove in part. Should the Government be successful, the water front that the city offers above will be appreciated because of its bold banks and handy channel." In a letter to the Harbor Commissioners, dated February 13, 1886, the Advisory Board says: "The portion of the (port-warden's) line above Richmond has been located with due reference to the works of channel improvement undertaken by the General Government above Petty's Island. The improvement, while it does not conform in plan and disposition of works to that suggested in 1883 by your Advisory Board, promises to lead in the same direction, and inspires much hope of success.†

In his annual report for 1883, General Weitzel, U.S.E, then in charge, said, "It will be a matter of great expense and difficulty

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cases the docks are so badly dredged that deeply-laden vessels are in peril of being strained to pieces with the flow and ebb of the tide. At the same time wharf property has risen in value until a twenty-foot front anywhere along the river would bring at public sale not less than $30,000.

Among the large steamers lying in the river awaiting their turn to discharge are the *Kairos*, *Hungaria*, *Cid*, *Regina*, *Romanby*, *Parklands*, *Acuba*, *Pensher*, *Roseville*, *City of Newcastle*, *Cairo*, *Haverstoe*, *Madrid* and *Earnmoor*, besides a large fleet of sailing craft, composed mostly of ice schooners, with an aggregate capacity of 30,000 tons. On the steamships enumerated there are afloat 50,000 tons of iron ore alone. The Spanish steamship *Leonora*, from Havana, which arrived in port last night, with over 4,000,000 pounds of sugar, was unable to go into her dock, and was swung around lengthwise in the river, blockading the ends of three piers.

* February 25, 1886.

† See the *Supplement* to this paper.

to obtain a channel under Five Mile Point, sufficiently deep to meet the requirements of this up-river commerce."

Captain Heuer, U.S.E., in charge in 1884, reports in substance as follows: "The plans of the Advisory Board contemplated a bulk-head, wing-dam or obstruction of some kind, about 3,000 feet in length, from Fisher's Point, on the New Jersey shore, to deflect a large portion of the ebb tidal flow." * * * "To meet the increased volume of tidal flow on the Philadelphia side, and avoid too serious a scour in the wharves, it would be necessary to cut away a portion of one side of Petty's Island, and build up on the opposite side of the same, putting a bulk-head around portions of the new shore lines of the island.

"The first step would be to build out a dike from Fisher's Point, as far as necessary, and watch the effect on the channel between Petty's Island and Philadelphia; it would certainly deflect an increased tidal flow down the channel. It might scour close to the wharves on the Philadelphia side, and endanger these, and to prevent this it was deemed necessary to cut away the lower end of Petty's Island." The approximate estimate for this improvement is put at $1,010,460, of which but $30,000 are for the dike; the balance, or $980,460 are then intended to counteract any injurious effects it may produce.

The suggestions reported to General Weitzel, in November, 1883, contemplated closing the south channel by a dike extending from Fisher's Point to the head of Petty's Island, and thus throwing the whole of the ebb prism into the north channel. This, it was estimated, would require a sectional area of about 55,000 square feet, and "necessitate the removal of upwards of 6,000,000 cubic yards of material."* At thirty-five cents per yard, this one item would then be over $2,100,000, exclusive of the cost of the dike, land-damages, etc.

From this it appears that the Five Mile Bar is the key to the upper river, not only for general but for local commerce, and the opening of a suitable channel through it will render a large and valuable frontage, immediately available, yet the expense, according to the plans proposed, is almost prohibitory. A consideration of the physical features of the bar, the causes of and the efforts to remove the same, with some suggestions as to improved and more

* *Report of Chief of Engineers*, 1884, vol. i, p. 868.

economical methods of accomplishing this end, become therefore questions of great commercial and financial interest to the City as well as to the General Government.

DESCRIPTION OF FIVE MILE BAR.

This deposit of fine sand forms a submerged ridge, extending from a point in the Pennsylvania channel below the head of Petty's Island, in an easy curve to Harrison's Wharf at Five Mile Point, a distance of about 8,000 feet. It occupies the neutral ground between the paths of the flood and ebb resultants, and being composed of light material, is readily affected by the opposing forces to which it is subjected. These are the flood and ebb tidal prisms, and the fresh water drainage flowing in different paths and with variable intensities as affected by the form of the river bed above and below the bar.

How these forces have been modified by various artificial changes in the banks of the river can best be seen by examining their effects upon this plastic mass of material at different dates and comparing notes as to position, quantity and direction. Hence the great value of the accurate comparative surveys which the Government has made at various times. Such a comparison to be thorough and reveal the whole truth should be made with reference.

(1.) To the *linear features*, as the crest line, the contours, the thalwegs and the slopes.

(2.) To the *areas*, as showing the relative sizes of the sections, not only of the bar, but also of the fluid prism constantly playing upon it.

(3.) To the *volumes*, as showing the mass movement in cubic measure.

(4.) To the *dynamics*, as indicating the measure of the forces and resistances, their lines of direction, degree of relative permanence or variation, and duration of action. The resultant effect being the *difference* of movement produced by these forces, hence the slowness of the changes in tidal rivers.

" In alluvial bottoms the channel is the cast of the living waters within; it is not the independent mould into which the river is turned, but it is the trough hollowed out by the running water, and it alters its form and direction with every change that may be induced upon the flow." *

* G. B. White, Captain U.S.N.

In subjecting the bar in question to this scrutiny, it becomes necessary first to state the simple mechanical law by which the effect may be measured.

THE MATHEMATICAL PRINCIPLE.

If a flexible but straight line *A B* be acted upon by a series of equal and opposite forces in any direction, the line will remain straight, but if any of the forces be augmented or diminished in intensity, the line will become bent in a direction away from the greater force until the equilibrium is restored or the line broken.

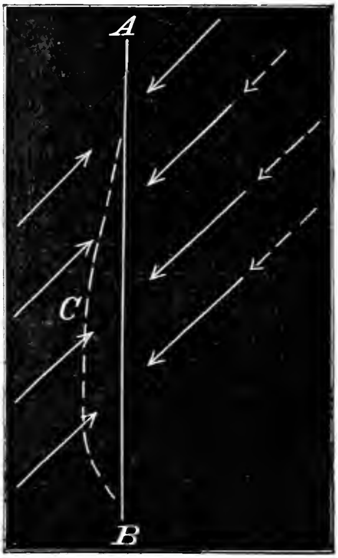


FIG. I.

Thus, in the diagram (*Fig. 1*), if a system of forces in equilibrium be increased by the amounts represented in broken lines, the straight line *A B* will become flexed in the direction of *C*. The same effect would result from a decrease in the intensities of the opposing forces. It is thus that the flexure of a curve as shown from a comparative chart will indicate the changes that have taken place in the forces producing these effects.

THE APPLICATION TO THE CREST OF THE BAR.

An examination of *Plate III* will show at once the marked changes that have occurred in the form and position of the axis of Five Mile

Bar, since the survey of 1843. At that date the concavity, as shown in the dotted curve, was greatest, indicating an excess of pressure from the flood tide rolling up the northern channel. During the intervening years the crest line has been gradually flattened out until the direction of its curvature has been reversed. The greatest movement having been about 810 feet. The diagram shows the relative amounts and dates of these changes.

These effects may be due to a diminution in the flood forces, an increase in the ebb, or a change in direction of either caused by auxiliary works.

As by far the larger part of the volume of the stream is composed of tidal waters, and as there is nothing to augment this volume from year to year, but on the contrary much to diminish it, we may presume at once that the ebb forces have not increased but that there has been a sensible diminution of the flood since 1843, whereby its velocity at this point has been so diminished as to produce deposits and to permit the neutral axis to recede. We must seek the cause for this effect down the river, and have not far to go, for, on examining the earlier and later maps, it will be seen that at the lower end of Petty's Island, the north or flood channel has been seriously throttled by the extensions of the pierheads on both sides, more especially by those at the Port Richmond coal wharves and below. In response to inquiries as to the dates of construction of these wharves Mr. H. K. Nichols, Chief Engineer of the P. & R. R.R. Co., replies that "the northern tier of wharves was finished in 1843." (These are the shortest wharves, and from this it appears that they were built in the year of the survey. They are shown on the chart.) He adds:

"To the south, including No. 14, finished in 1845.

"In 1854, finished to and including No. 18.

"In 1862, Nos. 4, 6 and 8 extended." (How much is not stated.)

"In 1865, No. 14 extended." (How much is not stated.)

"In 1866, Nos. 19, 20 and 21 completed.

"In 1867, Nos. 13 and 18 extended. Several other extensions were made, the dates of which were not found."

The actual condition of these works and their relation to the channel will be seen on *Plate II*, taken from the U. S. C. S chart of 1878, scale 800 feet to 1 inch.

The wharf of the Kensington (City) Water Works, which extends to a considerable distance, was built about 1849. That of the Port Richmond Iron Works (I. P. Morris & Co.*) was built in 1839 and extended twice; the first time in 1850, 100 feet, and again in 1869, 187¾ feet, making a total extension of 287¾ feet. The original wharf extended about 520 feet from Bank (now Beach) Street. It is now 760 feet beyond the east side of Beach Street and about 380 feet beyond the low water line of 1847.

The wharf of Wm. Cramp & Son, at the foot of Norris Street, extends to a distance of 612 feet from the east line. It was prolonged 156 feet 9 inches in 1870 to the present port warden's line, which was established April 5, 1849. In 1843, none of these wharves were completed.

In addition to these extensions, there are others on the north bank of Petty's Island which have aggravated the injury to the channel above by materially reducing the flood volume and so weakening it as to destroy the former position of equilibrium between flood and ebb at the crest of the bar.†

This movement is also shown by the contours and sections which have followed the general direction of the crest. The various fathom curves of the north or flood channel have fallen back about 1,000 feet, while the corresponding south channel contours have advanced as the ebb wedged its way between the bar and the south shore, eroding and compressing the southern slope of the bar.

The cross-sections (*Plate IV*) taken on the line *A B*, of *Plate II*, at intervals of 1000 feet exhibit the shifting in the position of the fulcrum and weights, as in a scale beam. In section No. 1 there was no trace of the bar in 1843, and the flood channel was nearly the same as the ebb, there being only a difference in the slope of the two banks. In section 2, the terrace is seen at about 12½ to 13½ feet depth. In No. 3, a low bar with 12 feet of water, flanked by channels of equal depth. In the succeeding sections the bar becomes higher and the flood channel deeper and wider, reaching its maximum in section No. 5, 5,000 feet below the point.

* Information obtained through the courtesy of Mr. John T. Morris, President.

† On May 8, 1883, Prof. Henry Mitchell estimated the reduction of area from this cause to be eleven per cent., "with very little change of channel depth."

In the later surveys, the rapid growth of the bar, increase of ebb, and diminution of flood sections are so clearly shown as to require no further comment at this place, where it is intended merely to call attention to the relations between the flood and ebb forces.

Measurements of these sections by the planimeter also show a gradual diminution in sectional area of the water prism; that is, notwithstanding the great variations in width, there has been a shoaling which increases in the down-stream direction, indicating a diminution of the scouring forces as the head of the island is approached.

The accompanying table is confirmatory of these conclusions and contains many valuable suggestions; for example: Columns 3 and 4 show that the filaments of *maximum* flood and ebb cross each other between sections 1 and 2 (see chart, *Plate II*) and that the ebb is greater at section 1, and slightly less in 2 and 3. In section 8 it is the greater in the south channel where it reaches the unusually high value of 4·52 feet per sec., and the flood is greater in the north channel, 400 feet from shore. In column 5 it is observed that the *mean* velocities of the ebb are greater than those of the flood excepting at No. 8 north, where the mean flood velocity is 2·98, and the ebb 2·90. Hence it is safe to infer that *the volume of the ebb may be somewhat increased through this section* without producing any serious damage to the pier heads or to the channel, and without increasing the section by artificial means, but on the contrary, the result of such increase of volume would be highly beneficial. To increase the section for a small increase of ebb volume would prove injurious, as it would reduce the working power of the stream. It is also evident that there is an excess of ebb volume passing through the south channel, a portion of which may be deducted without injury to the latter, and with great benefit to the north channel.

Comparing the total area of section 8 (north and south) with those of 1, 2 and 3, as stated in column 9, it appears that at both flood and ebb, 8 is 20 per cent. greater than 1; 22 per cent. less than 2, flood; 20 per cent. less than 2, ebb; 24·8 per cent. less than 3 flood, and 23 per cent. less than 3, ebb, so that section 8 as a whole is more than large enough to pass all the water that reaches section 1, but not large enough to supply sections 2 and 3, and still maintain its velocity, hence the shoaling in the reach.

TABLE SHOWING THE ELEMENTS OF THE CROSS-SECTIONS AND DYNAMIC CURVES AT AND NEAR FIVE MILE BAR, DELAWARE RIVER, PHILADELPHIA.*

1	2	3		4		5		6				7		8			9		10		11		12
Sections	Widths at Maximum Flood Tide.	Maximum Velocities reduced to Mean Tide. (In Feet per Second.)		Distance from Origin to Point of Maximum Velocity. (In Feet.)		Mean Velocity. (Feet per Second.)		Distances to Loci of Mean Velocity, or, Location of the Principal Working Part of the River.				Width of Portion of Stream, in which the Velocity exceed the Mean. (Outside Lim.)		Depth at Time of Maximum Velocity in Feet, below Mean Low Water.		Distance from Origin.	Area of Sections in Square Feet.		Distance to Line of Mid-Areas in Feet.		Distance to Axes of Mid-Volumes.		Remarks.
No.	Ft.	Fl'd	Ebb	Fl'd	Ebb	Fl'd	Ebb	Flood.		Ebb		Fl'd	Ebb	Fl'd	Ebb	Ft.	Fl'd	Ebb	Fl'd	Ebb	Fl'd	Ebb	
1	2550	3·11	3·57	2000	1300	2·61	2·81	542	2311	831	2078	1769	1247	42·25	41·50	1900	51825	49922	1590	1590	1613	1575	The maximum flood is 0·75 foot higher than maximum ebb. Hence, the difference in depths, areas, etc.
2	5820	3·17	3·13	1300	2400	2·05	2·20	158	270	900	3388	2542	2488	28·35	27·60	2500	76172	71822	2575	2585	1841	2404	
3	5900	2·85	2·79	1500	3000	1·95†	2·17	882†	4223	912‡	4522	3341	3610	31·05	30·30	1200	77806	73447	2664	2649	2254	2625	
8 N.	1350	3·38	3·30	400	1000	2·98	2·90	214	934	280	1025	720	745	37·00	36·25	100	33865	32785	504	504	520	542	On north side of Petty's Island.
8 S.	1900	2·88	4·52	600	700	2·35	3·26	233	922	390	963	759	573	36·10	35·35	600	28485	27045	790	790	733	783	On south side.
	3250	3·13 mean	3·91			2·66	3·08										62350	59830					

* Collated and extended from the data collected and computed by Mr. Marindin, Assist. U.S.C. and G. Survey, and embodied in the Report of Prof. Henry Mitchell on *A Physical Survey of the Delaware River*, Appendix No. 9, *U. S. Coast. Survey Report for 1878.*

† As the flood passes up over Section No. 3, the portion of its prism, 307 feet wide, lying between 2,100 and 2,407 feet from shore, has its velocity diminished to a minimum of 1·76 feet per second in passing over the foot of the bar.

‡ The ebb at this section passes several times from maximum to minimum values, thus dividing up into three streams.

The times of transit through the north and south channels are also different, producing some interference and deposit at both ends. Columns 10 and 11 show on which side of the mid-area the loci of mid-volumes of flood and ebb are situated, and the great divergence between these latter resultants in sections 2 and 3. In 1 and 8 their paths are more nearly coincident, and hence their useful work greater.

It will be seen that the loci of mid-volumes cross between sections 1 and 2. In 1, the locus of the resultant of the ebb stream is thirty-eight feet to the north of that of the flood, in 2 it is 563 feet to the south, and in 3 it is 371 feet distant but on the same side.

Again, if the limits of mean velocities of flood and ebb be located, as given by their co-ordinates in Column 6, it will reveal the position of the more vital part of the stream lying between them. Its breadth is stated in Column 7, where it appears that at section No. 1 the major part of the flood is 522 feet wider than the ebb.

At No. 2, these prisms are of nearly equal width, although their resultants are widely separated, being, as stated, 563 feet apart; the flood axis running near the crest of the bar, the ebb over the thalweg. What is needed there is a structure that will draw in the ebb and deflect a portion of its volume to the other side. This is more fully confirmed by an examination of section No. 3. Here it is found that the flood in the north channel is divided, by the foot of the shoal, into two parts, in which the velocity exceeds the mean (1′·95). The minor prism passing over the shoal is but 307 feet wide, and has a velocity diminishing to 1′·76 at 2,200 feet from the Pennsylvania shore. The ebb in the same (north) channel is also divided into two streams, having velocities above the mean (2′·17), located as follows: The smaller one, between 912 and 1,444 feet from shore, with a maximum velocity of 2′·81 at 1,200 to 1,300 feet. This 100-foot prism has the greatest velocity in the section. The larger ebb prism lies between 2,183 and 4,527 feet from the origin and is consequently nearly half a mile wide. It is divided near its northern limit by the head of Petty's Island (at 3,000 feet), where the greatest velocity, 2·79 feet, is found for a width of 800 feet; that is, from 2,600 to 3,400. This causes a small portion of this ebb prism to traverse the region just north of the island, and maintains the nar-

row channel it has created along its path. Between these two more rapid streams lies a retarded prism, extending from 1,444 to 2,183 feet, or 739 feet wide, in which the velocities diminish to 1'·79 at 1,600 feet out, due to the bar over which it has just passed. These phenomena are characteristic of streams undergoing contraction and are produced by the reaction of the banks in causing an increased velocity near them, which produces the scour near shore and the low spur in or near mid-stream. The remedy here then consists in throwing a larger volume of the ebb into this shore channel along Petty's Island by a reaction dike, at the head of the island, which shall draw off a part of the ebb from the south channel and cause it to traverse the northern one. Such are some of the deductions from these data, which are believed to be sufficient to explain the physical phenomena of the changes produced at this site and to enable the engineer to apply the remedy.

HISTORY OF THE IMPROVEMENTS.

As the lessons of experience are of great value in projecting works and avoiding unnecessary expense, it becomes an important element to ascertain what has been done, if anything, and what have been the results. To this end the history of the work is indispensable.

In the *Report of the Chief of Engineers for 1884*, p. 794, the statement is made by the resident engineer, then in charge, that the improvement of Five Mile Bar is an important but

"At the same time a very difficult one to treat, owing to misjudgment on the part of the city authorities of Philadelphia, having allowed the wharf line to extend far beyond its proper position in rounding the point. Many wharves have been built out to this line, and have proved a serious source of shoaling in many parts of the channel below. A curtailment of these wharves will probably be a part of any thorough scheme of improvements, but as this would entail very great outlays of money at the present time, other means have been employed to enable the commerce of the river to pass this point, viz., cutting a channel during December, 1881, and January, 1882, at the upper end of Petty's Island. This channel was subsequently widened to 200 feet, with an allotment of $4,500. It should be deepened to twelve feet at mean low water, and an appropriation of $5,000 should be made for this purpose."

The premises in the above statements being at variance with the facts, the conclusions must be at fault, as there are believed to be but two short wharves at Five Mile Point, and these are not

used to any extent, nor are they thought to have produced any serious shoaling in the channel.

In 1881, a channel was dredged across this bar to a depth of 9 feet, with a width of 75 and length of 700, at a cost of $2,500. The price was $38\frac{9}{10}$ cents per yard. It was regarded as a temporary expedient only, and no degree of permanence was anticipated. In fact, upon investigation, at the end of the year, the channel was found to have shoaled to within one foot of its original depth.

Subsequently, the existence of a narrow but straight channel was discovered close under the upper end of Petty's Island. It was thought that if this were improved, it would, in all probability, maintain itself. It is believed to have been caused by the building up of the submerged end of Petty's Island by a bulk-head.

This channel was enlarged in 1881–82, to a depth of 9 feet, width of 100 feet, and length of 800 feet, by the removal of 6,500 cubic yards at 41 cents, making a total of $2,665. It was found to have silted up soon after to a width of 75 feet, and was widened in 1883, to 200 feet by the removal of 11,566 cubic yards at 35 cents, costing $4,c48.10.

These attempts to improve the channel by dredging having proved abortive, the Advisory Board to the Harbor Commissioners recommended three plans;* first, to close the south channel by a dike from Fisher's Point, and the cutting off of part of the north side of Petty's Island; second, the closing of the south channel, making a basin of that part of the river, and, third, the building of a short dike from the Jersey shore, and a cutting away of the north, and extension of the lower shores of Petty's Island, either of which would cost about $1,000,000. The plan recommended by the Government engineer was but a slight modification of the latter and was estimated at $955,290.

Again, in their *Preliminary Report on the Permanent Improvement of the Delaware River*, dated Philadelphia, January 23, 1885, the Board of Engineers say:

"The deepening of necessity must be done either by artificially dredging the shoal spots, or by increasing the power of the current at these points by contracting the channel, or by concentrating a larger volume of water where they occur, so that by its scouring action the obstructions may be washed away." * * *

---

* *Report of Chief of Engineers*, 1885, p. 805.

"After considering the various plans submitted for the improvement of the upper part of the harbor in front of the city of Philadelphia, each of which contemplates the cutting away of a part of Petty's Island, and after making a personal examination of the locality, the Board concluded that an increased tidal flow through the west (north) channel near the head of the island, sufficient to secure the increased depth and width required, could be procured by a deflecting dike, starting from Fisher's Point, and running down stream in a slightly curved direction, sensibly parallel with the pier-line of the Pennsylvania shore opposite, extending toward the head of Petty's Island, as far as might be necessary for the purpose of diverting into the west (north) channel a considerable part of the ebb tidal volume now passing down the east (south) channel." * * * "It is probable, however, that in the construction of this dike an injurious scour may occur at and beyond its outer end, to prevent this a mattress-sill of brush not more than three feet in thickness should be kept well in advance," etc.

Beyond the American Ship-Building Company's yards, "it is thought that a depth of twelve feet will be sufficient for the wants of commerce for a long time in the future."

Assuming the sill to extend all the way to Petty's Island, the Board's estimate for the dike was $87,570, and there was added for dredging $462,000, making a total of $549,570 for the improvements at this bar. The dimensions finally adopted for the dike are stated in a letter of the engineer in charge, under date of October 9, 1885, as published in the *Report of the Chief of Engineers for 1886*, p. 820, as follows:

"The dike is to extend from Fisher's Point, N. J., 3,000 feet toward the head of Petty's Island, with a mattress sill about 3 feet thick, projecting 500 feet farther, leaving an open channel way of about 3,000 feet between the end of the sill and the island." * * "The top of the dike is to be at the level of mean low water, allowing the tides to flow over it." * * * "The dike is evidently designed to throw more water into the channel west of Petty's Island. Work upon it was commenced in August, 1884, but stopped by an injunction in September."

The Board of Engineers, in reporting on the testimony taken during the injunction proceedings, under date of October 28, 1885, says: "It does not appear to the Board that there any other means available of successfully maintaining the requisite channel depth in this part of the Delaware River than those which they have heretofore proposed, and they recommend, therefore, that the construction of the Fisher's Point dike be continued and completed." This was accordingly done, and the dike was finished

in conformity with the plans and dimensions heretofore given, on June 30, 1886, at a cost of $39,560.28.

To expedite the formation of a channel, under the influence of the dike, a cut was made in the path of the ebb and flood currents across the bar in May, June and July, 1886, when 125,000 cubic yards of material were removed at nine cents, costing $11,250.00.

Of this work, the engineer in charge says in his report for 1886:* "The object of the dredging in progress is the formation of a channel twelve feet deep at mean low water, connecting the deep water above with the deep water below the bar. This will afford great relief to the commerce moving between Philadelphia and the river above. This channel is not supposed to be permanent, but its value is so great as to be worth some cost in maintaining it."

Thus, up to this date, there had been expended altogether in the efforts to improve this locality, $60,023.38.

THE EFFECTS PRODUCED BY THESE IMPROVEMENTS.

To determine the results of the dredging completed in July, 1886, and of the dike finished the month before, a re-survey was made in May, 1887, when it may safely be said that not a trace of the cut, which was made 125 feet wide, 2,500 feet long, and 12 to 16 feet deep, but ten months before, remained. How much sooner it filled up is not known, but as the material is very fine, and as a lateral pressure was produced by the dike, its closure was probably very rapid. By examining cross-section No. 2, which intersected the upper, and No. 3, the lower end of the cut, it will be seen, by comparing the surveys of August, 1886, and May, 1887, that it is entirely obliterated, and the same result is revealed by the charts of the soundings and by the contours.

The effect of the dike upon the bar is also shown by these cross-sections, as well as by the plan of the crest lines already referred to (*Plate III*). On cross-section 1, the crest of '87 lies to the left of all previous positions, and it is slightly higher, indicating increased shoaling at that place; on No. 2, it is slightly to the right, the point of inflection being between these sections, and it is about a foot lower than in '78 and '82 (that of '86 was cut down by the dredging); in No. 3, it is seen to the right of '86, with same depth,

* Page 805.

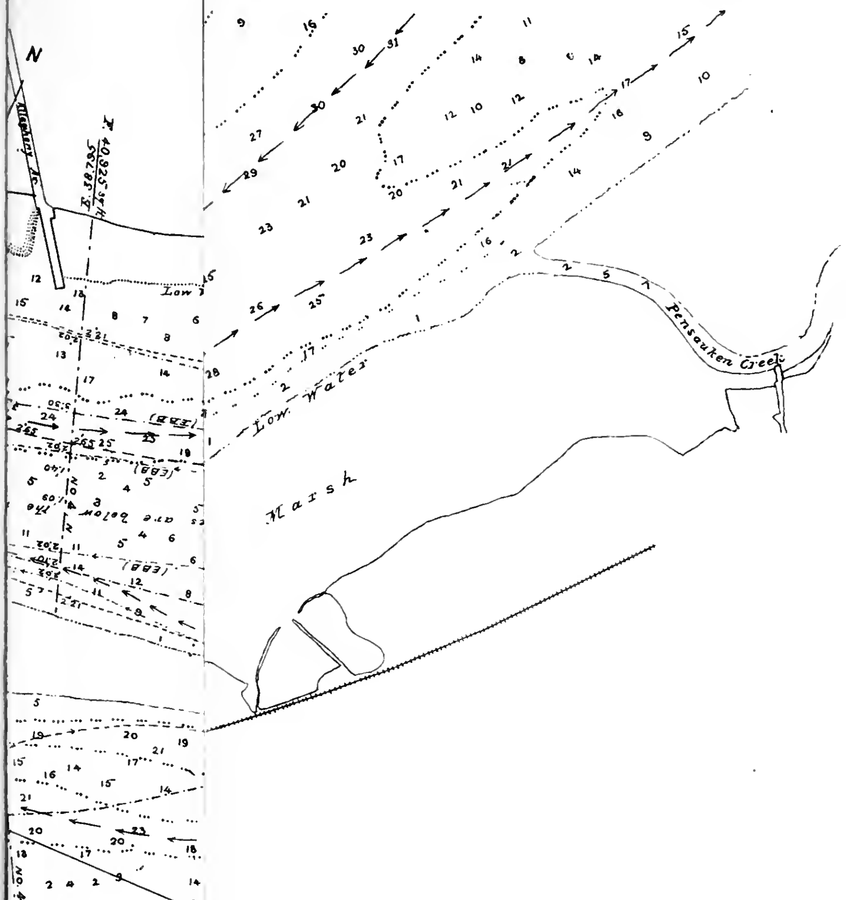
under '78 with a gain of a little more than three feet in depth, and a still greater gain over '82. During the interval since the completion of the dike, which is abreast of Sections 2, 3 and 4, however, there appears to have been a slight erosion from the brow of the southern, or ebb, slope, and a filling up of the cut.

The conclusion to be drawn from this is, that if a dike be intended to increase the scour through a cut, it should be placed at such a distance *above* it that the increased velocity may be acquired before the water enters the cut, and not abreast of it where the wedge-like action of the stream compresses its outer bank and tends to crowd it over into the cut.

In section No. 4, the surveys of 1886 and 1887 show the crests as nearly coincident, with an increase of deposit on the right or flood slope and shoaling in the flood channel. As compared with its position in 1843, there has been a northward movement of about 800 feet, and a loss in depth of three and one-half feet. In No, 5, below the end of the dike, there is a gain in depth on the crest of about one foot. The flood channel is very much contracted as compared with that of 1843. No. 6, shows practically no change from 1886; the salient end of the ebb channel terminates under the former crest of 1843. These cross-sections further show that in 1843, there was a good and wide twelve-foot channel on the north or Pennsylvania side of the river across the bar. In general then it may be concluded that, between the dates of the last surveys, the dike had produced a deepening of about one foot along a portion of the crest and had compressed or rolled the bar to the northward, at the point of maximum movement between sections 3 and 4, or abreast of the end about 150 feet, and changed the direction of its curvature from a line nearly straight to one which is concave. The greatest depth along the crest is now but eight feet for a short distance, with five-foot spots in the immediate vicinity.

THE PROPOSED REMEDY.

From the statements and opinions already cited, it appears that the resources of the projectors of the several plans have been limited to dredging or to current concentration by means of dikes, extending from the shore in such a direction as to canalize the channel and contract the water way, thus affecting to a greater or lesser extent the entire volumes or prisms of the flood and ebb, and



of the DELAWARE RIVER

Petty's Island, Philadelphia,

:hs, relative velocities and

:ed improvements.

OMPILED BY

S M. HAUPT

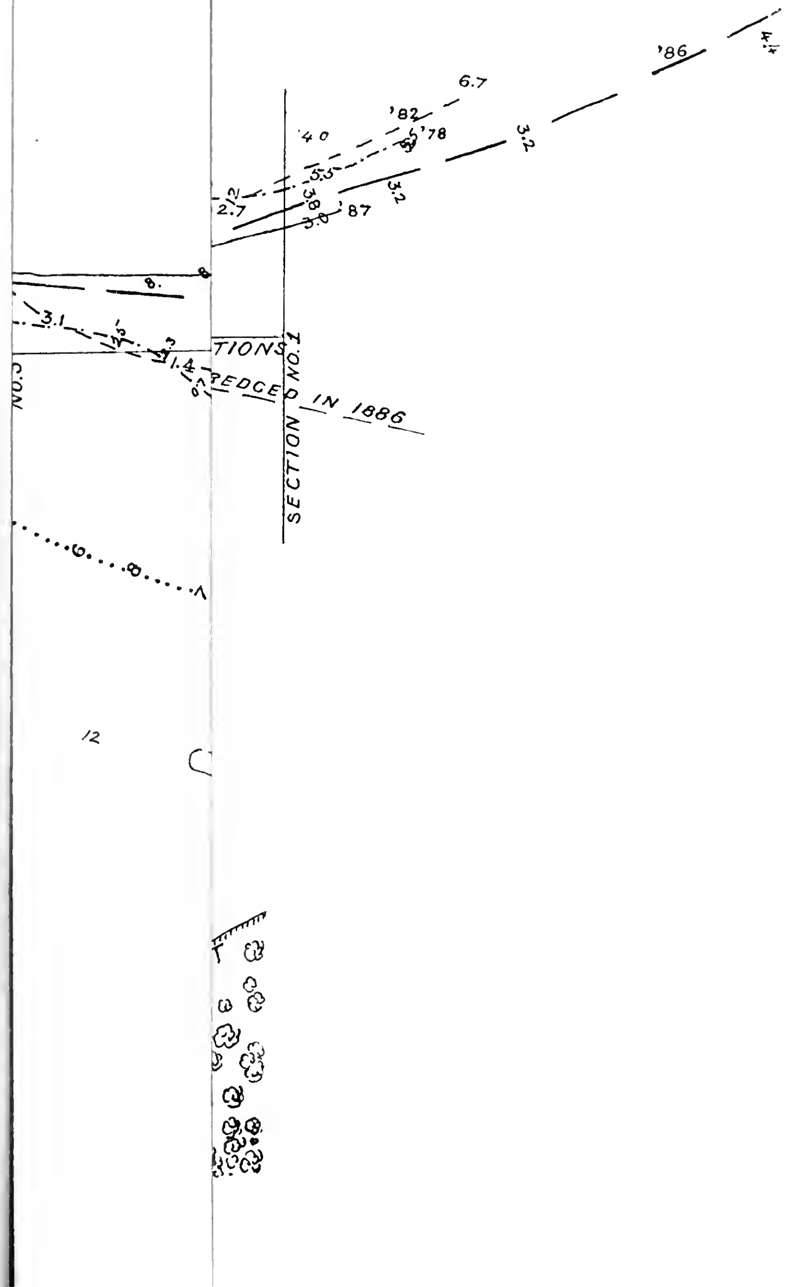
DYNAMIC CHART of the DELAWARE RIVER

In the vicinity of Petty's Island, Philadelphia, showing depths, relative velocities and proposed improvements.

COMPILED BY

LEWIS M. HAUPT

Plate III.



SECTION NO. 1
DREDGED IN 1886
TIONS
'86
6.7
'82
'78
3.2
3.2
'87
2.7
3.8
3.0
5.5
8.
3.1
1.4
12

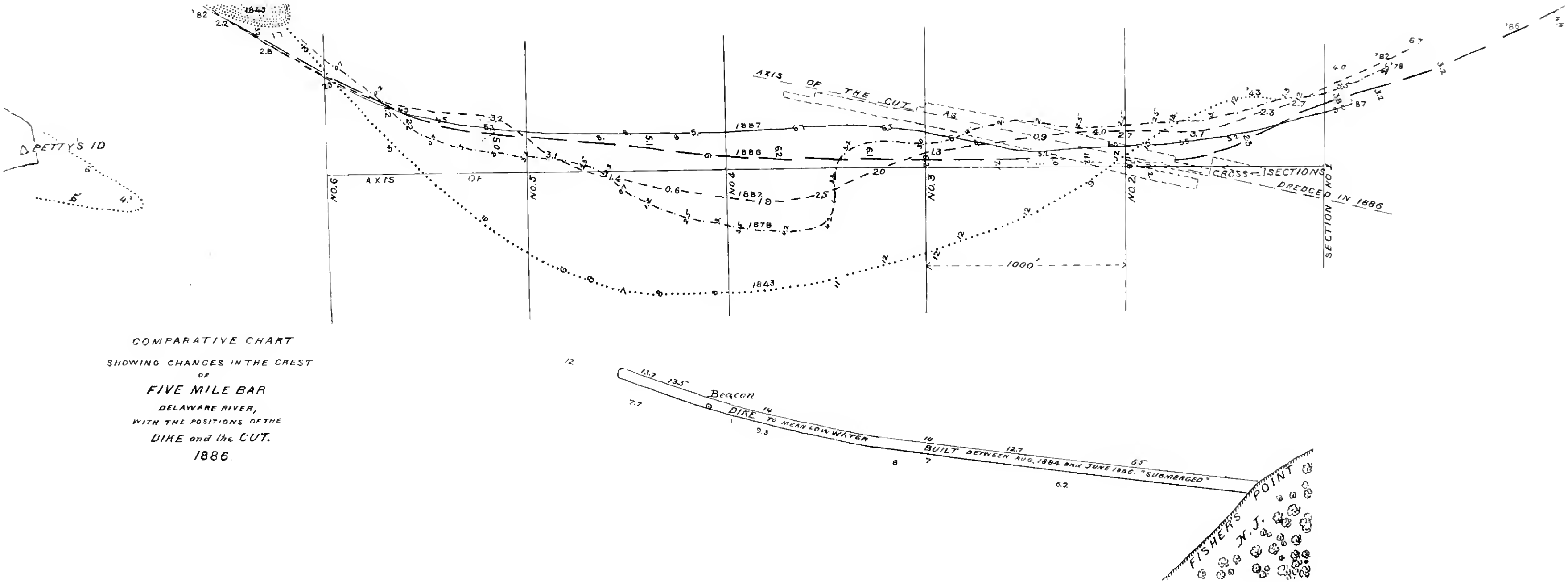
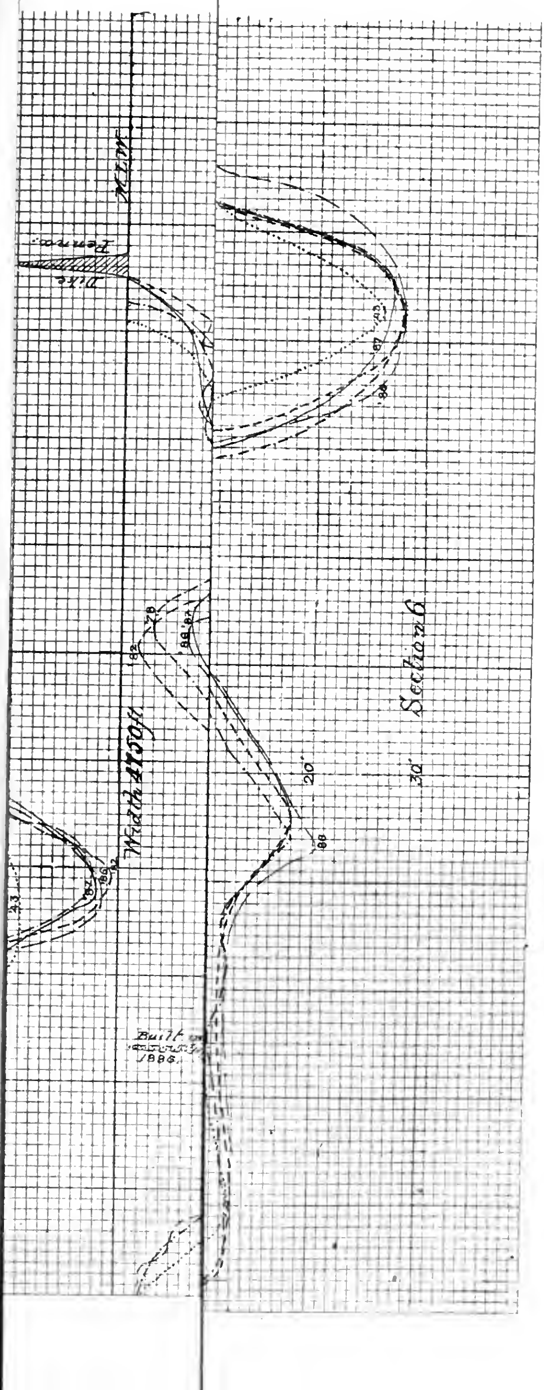
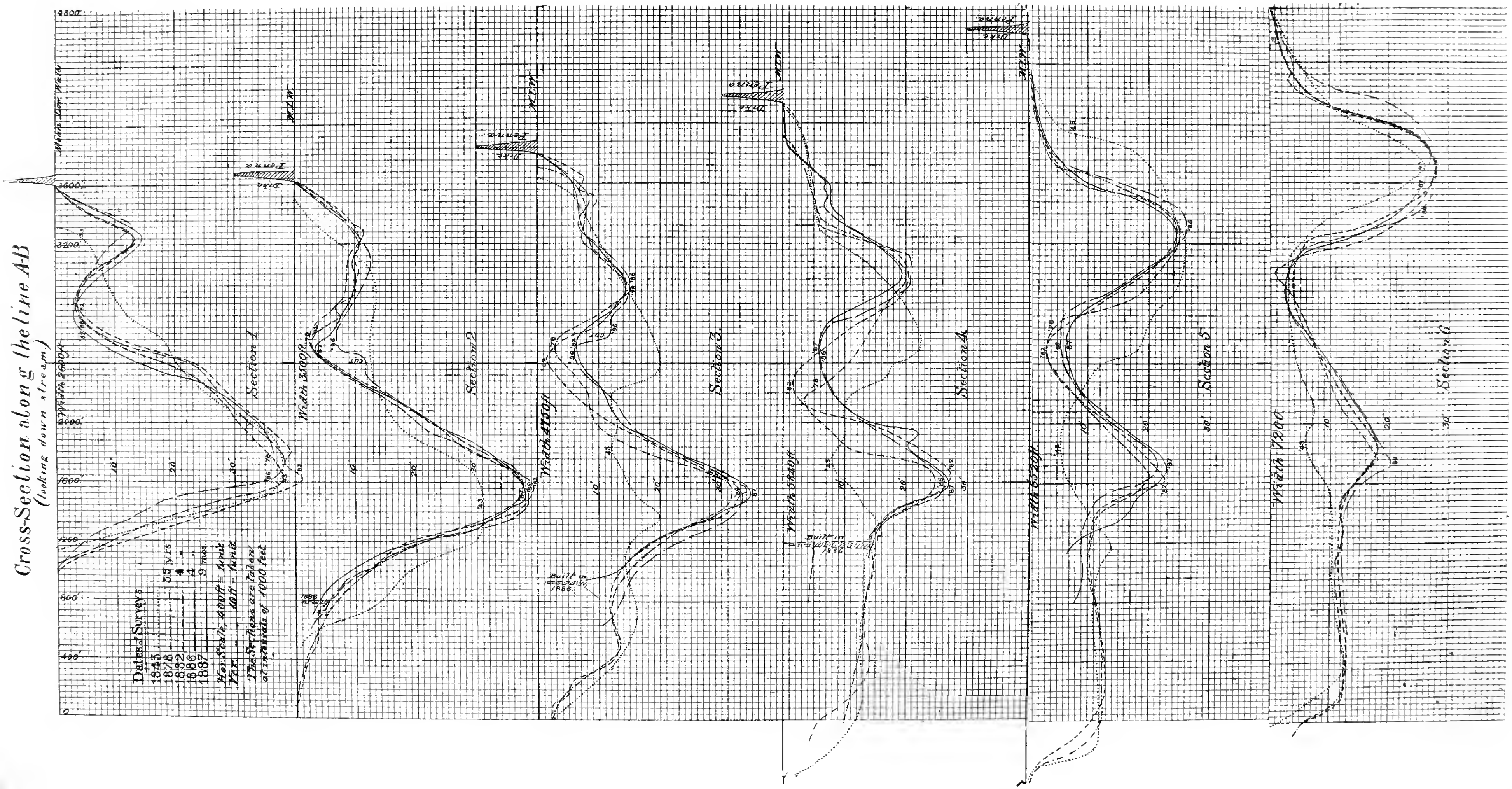

COMPARATIVE CHART
SHOWING CHANGES IN THE CREST
OF
FIVE MILE BAR
DELAWARE RIVER,
WITH THE POSITIONS OF THE
DIKE and the CUT.
1886.
PETTY'S ID
AXIS OF THE CUT AS
CROSS SECTIONS DREDGED IN 1886
AXIS OF
NO.6
NO.5
NO.4
NO.3
NO.2
SECTION NO.1
1000'
1887
1886
1882
1878
1843
Beacon
DIKE TO MEAN LOW WATER
BUILT BETWEEN AUG. 1884 AND JUNE 1886. "SUBMERGED"
FISHER'S POINT
N.J.

Plate IV.




Cross-Section along the line A-B
(looking down stream)
Dates of Surveys
1845 .... 35 yrs
1878 4
1882 4
1886 .... 9 mos.
1887
Hor. Scale, 400 ft = 1 inch
Ver. " 10 ft = 1 inch
The Sections are taken at intervals of 1000 feet
Section 1
Section 2
Section 3
Section 4
Section 5
Section 6
Width 2600 ft
Width 3100 ft
Width 4150 ft
Width 5840 ft
Width 6520 ft
Width 7200
Mean Low Water
M.L.W.
Dike
Penna.
Built in 1886

distributing their effects over the whole section included between the dike and the opposite bank. The water is thus deflected laterally in horizontal planes and tends to scour away the *sides* of its bed, which are opposed to this contraction, but the increased resistance also raises the general level of the stream and so produces a velocity *head*, which is the effective force in scouring upon the *bottom*. It is the *additional* momentum, due to the resulting increased velocity, that produces the scouring upon the bottom, and this is relatively more effective near the sides of the stream than at the centre. It is moreover, evident, from the observed velocities, that there is a mean current over the bar amply sufficient to remove any material in that vicinity; and yet the bar remains, because of the equilibrium between the flood and ebb movements, and because the bottom velocities are but feeble as compared with those on the surface.

The whole problem, as it presented itself to me, so far as the urgent and immediate needs of commerce were concerned, required the removal only of a *sufficient amount* of material from the crest of the bar to afford a passage for vessels drawing less than twelve feet of water, and *not the removal or disturbance of the entire bar.* This result, I believed, could be accomplished in one of two ways, both of which are local and direct in their application and neither of which can be injurious to the other parts of the river.

The methods both embody the principle of the utilization of the surface currents, either by a vertical deflection or by reaction, in combination with a lateral compression and concentration of a portion of the ebb and dispersion of the flood thus producing a greater *difference* in the amount of the flood and ebb movements of the particles than exists in a natural or unimproved condition.

The means taken to effect these changes are quite different; the one, being of a movable and temporary character, the other, permanent.

The first consists of a series of wings or gates, which swing about a horizontal axis at or near the surface of the water. The gates are hinged to caissons, scows, or any suitable floats which may be anchored in any position and at any angle to the currents. The gates are also ballasted to any extent dependent upon the pressure due to the velocity, and are left to do their work. As it was doubted whether or not the water would pass under, rather than

around, the ends, and as it was desired to have the data from actual experience, without any conjectures, arrangements were made to test the efficacy of this principle on the crest of Five Mile Bar, where the dam would not obstruct navigation, and accordingly a limited company was organized and a plant erected during July of this year (1887).

EXPERIMENTS WITH CURRENT DEFLECTORS AT FIVE MILE BAR.

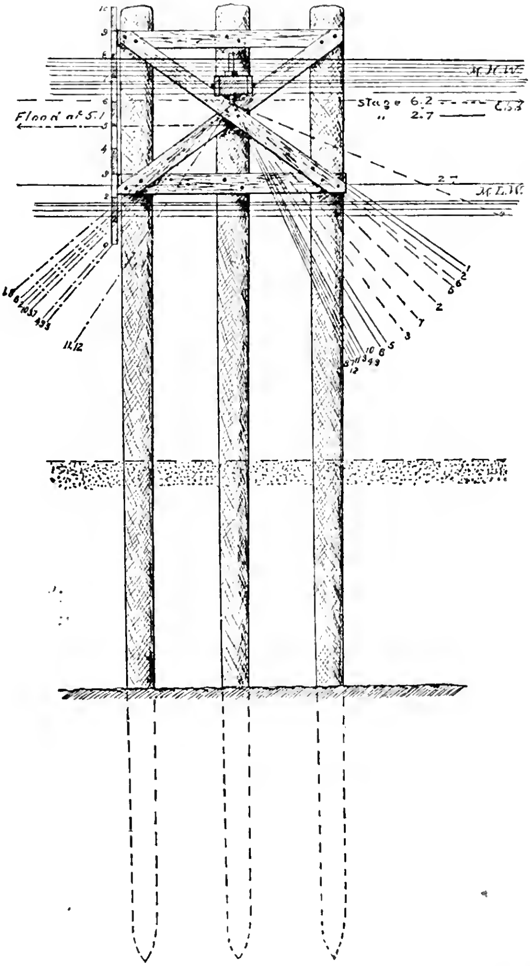
Two wings were put in position, the left one on the south side being 140 feet long and containing seven gates; the right wing 100 feet long with five gates, and leaving a channel way between them of 135 feet. The axes of the wings made an angle of 135°, that is, an intersection angle of 45° for the purpose of increasing the ebb prism through the channel.

The gates were twelve feet in depth, and were suspended so that at low water the lower edge was about eight feet below the surface.

Each gate was loaded with about 1,600 pounds of ballast, and, as a matter of economy, the supports were made of pile bents (as shown in the side elevation *Fig. 2*) instead of floats for this experiment. The deflectors scoured out a depth about equal to the distance from half tide to their lower edges, and increased the original depth from its average of nine to eighteen and twenty feet. This was more than I had anticipated, as it was only at the stand of the tide that they hung vertically, and I was still more surprised at the rapidity of their execution. Scarcely were they launched before the action began on the bottom, and within a few tides their immediate work was done. What the indirect effect will be upon the channel between the wings remains to be determined, as it is a question of time. Theoretically, the material from these elliptical basins would be deposited in mounds on either flank, which should aid in trailing the ebb, whilst the bottom material in the channel, being left somewhat unsupported, should gradually flow off laterally, and be carried to the mounds. The experiment confirms the supposition that such a floating plant would make an excellent current dredge, where there are sufficiently deep pools into which the dislodged material might settle without injury, as frequently occurs.*

* These devices are secured by Letters-Patent No. 339,548, issued April 6 1886.

The other or permanent device is what I have called a *reaction dike*, and is intended to be an aid to navigation, being both a day and night beacon, as well as a means for deepening and improving channels. It is, briefly, a permanent structure of such *form* as to



From half tide to bottom of gates is equal to the depth of the scour.

Original bottom (*a*).

After two days.

Average bottom.

Fig. 2. Section Showing Position of Gates at Different Stages of the Tide.

catch, compress, deflect and train a sufficiently large prism of the ebb to give the required depth which is obtained by producing a reaction of the mid-stream velocities upon the bottom. It is so placed, also, as to defend the channel from the deposits carried up

by the flood and thus increase the differential movements between these opposing currents. Its length will be about 1,000 feet, which is much less than that of the ordinary in-shore dike, and its effects much greater on the particular part of the obstruction to be removed, hence the cost will be reduced and the efficiency increased.

If the present dike at Fisher's Point be raised to high water, it would involve an increase of base and general enlargement of cross-section. If the average height of the present submerged dike be taken at twelve feet, to add six more, the sections remaining similar, would increase the area

$$\left(\frac{18}{12}\right)^2 = \left(\frac{3}{2}\right)^2 = 2\tfrac{1}{4} \text{ times,}$$

and the cost in the same ratio. This would make the high water dike cost about $50,000 more. The effect of such an enlargement on the present line of work, upon the bar 2,000 feet away, and across the deep-water channel, would be very slight, whilst the same amount expended on a structure situated directly upon the site where the channel is desired, would be more effective, definite and expeditious.

In the lowest of the estimates for improving the channel across Five Mile Bar, there was also an item of $462,000 for dredging, to provide for the supposed increase of the ebb discharge through the north channel, thus greatly augmenting the cost of the work. The reason that dredged channels are not self-sustaining arises from the fact that whilst the area of the water way is enlarged, the prism of water remains constant, hence its velocity and consequent scouring force is diminished and the cut fills up. But a *change in the form* of the bed whereby it is deepened, locally *without increasing the area of the cross-section*, will maintain a permanent channel. This can readily be accomplished by the reaction-dikes and deflectors already described.

There are other radical improvements projected for this port, which involve a very large expenditure, but the length of this paper prevents their consideration for the present.

University of Pennsylvania.
Philadelphia, September 21, 1887.

SUPPLEMENT.

After the above-named paper was completed, there was handed me a printed description of the plans suggested by the Advisory Commission, as read at their meeting, held May 8, 1883. As this is authentic and explicit, I have appended it as a part of the history of the studies as to this part of the river. The record reads :

"After studies of the probable effect of deflecting works at and below Fisher's Point without satisfaction, because of the difficulty of accommodating such works to both ebb and flood, it is proposed *to extend the eastern point of Petty's Island,* not only over the ground lost since 1843, but beyond, so as to control the proportion of the ebb which shall seek the channels below. The crude conception of the work is a dike of fascines or cribs extended *in a straight line towards the extremity of Fisher's Point or to the left of same,* so as to leave about the same sectional area on either side, as things are now. This dike would fall to southward of the split of the ebb current, and to the *southward of the line of* MID-VOLUME, so that it would induce a greater proportion of flow to the northward of Petty's Island during ebb, without disturbing the relations of flood volumes, or injuriously affecting the canalized southern channel ; indeed, this dike would form a part of the project for the south channel, and to its advantage, since it would probably arrest the shift of deposits which, since 1843, have caused the thirteen and one-half feet channel to wander among the shoals, and may, possibly, increase the channel depth. *It would not be less than 2,600 feet long beyond the low water point of the island.*"

The extents and locations are stated in the parts I have italicized. The object is "to control the proportion of the ebb which shall seek the channels below" and "induce a greater proportion of flow to the northward of Petty's Island during ebb, without disturbing the relations of flood volumes." It will thus be seen that the conclusions reached in the body of my communication as to the site for and object of the works are identical with those stated by the Advisory Commission more than four years earlier, yet the plans which have been executed are those to which the Advisory Board gave but a qualified approval, based upon hope of success. As to the *means* of producing the desired effects, there appears to be a material difference, for if the dike as above described, be plotted as shown on *Plate II*, by the heavy dots extending 2,600 feet from low water at the eastern end of the island in a straight line to the extremity of Fisher's Point, it will not leave the curve of mid-volumes to the north of it, but will intersect it a short distance from the island and leave it on the

south, so that the area and volume of the ebb section and prism on the north side will be diminished and the proportion of the ebb stream flowing in the *south* channel would be increased, to the further detriment of that on the north side.

The *reaction dike* (*Plate II*) jutting out from the head of the island would have a development of but little more than one-third the length of the straight one, and would withdraw a prism discharging about 15,000 cubic feet per second, or more, if it be found necessary, from the south and project it into the north channel, thus increasing the ebb velocity of the portion near Petty's Island to over three feet per second, and giving a straight channel of sufficient magnitude to satisfy all the present requirements of commerce. By extending the dike, future demands may readily be met by the same means.

University of Pennsylvania, October 7, 1887.

DESCRIPTION OF AN IMPROVED TRIPLE-EXPANSION ENGINE.

By Wm. M. Henderson.

[*Read at the Stated Meeting of the* Institute, *held Wednesddy Sept. 21, 1887.*]

Mr. President, Ladies and Gentlemen:—The latest innovation in steam engineering has been the introduction of triple-expansion engines. About the year 1863, being then in the South Pacific, I remember what a furore was cerated by some new steamships sent out from the Clyde by Messrs. Randolf, Elder & Co., having compound or double-expansion engines, which effected a saving in fuel of about twenty-five per cent. over the existing single-cylinder engines; notwithstanding the fact that the engines in use were fitted with approved expansion gear. These compound engines, as most of you are aware, had two cylinders; the second cylinder being about twice the diameter of the first. Engineers of eminence are not wanting who still insist that all the advantages of the compound engine can be secured in the single cylinder type, provided the total ratio of expansion in the two engines is the same. Facts prove this not to be the case, to which my single cylinder friends reply, "So much the worse for the facts."

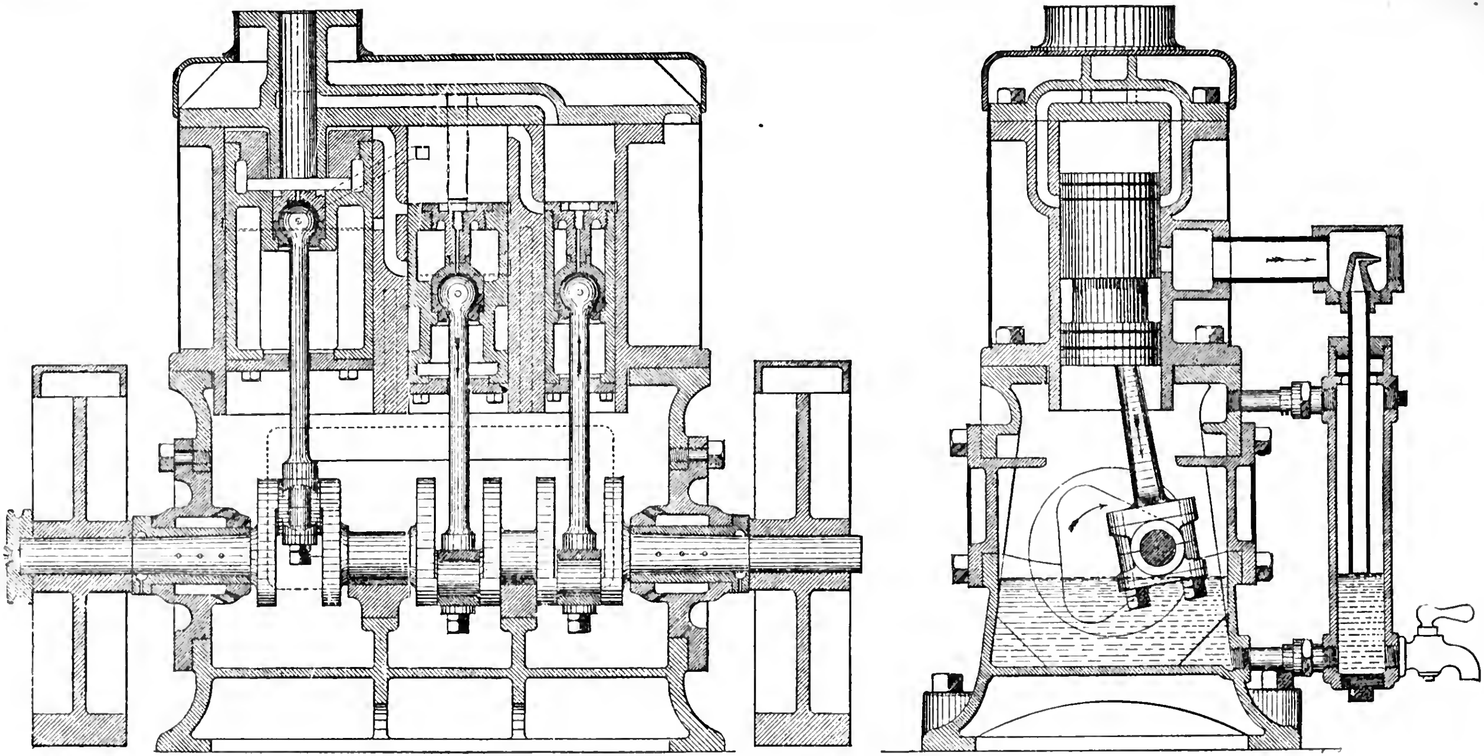
The compound engine reigned supreme for about twenty years, when another mechanical departure took place in the production of the triple-expansion engine, which overcame the double-cylinder engine to the same extent that the double had surpassed the single-cylinder engine. Another advance of twenty-five per cent. in the economy of fuel. These are facts reiterated in every mechanical journal of the day. To endeavor to explain the causes bringing about this result would occupy too much time at this meeting. But it may all be accounted for in using higher pressure and causing the work of the steam to be performed in separate cylinders, where the difference of the initial and the final temperature in each is divided into gradual stages; and not compelling the extremes of temperature to occur in the same cylinder, where the refrigerating effect of the walls, left cold by the exhausting steam, meets the hot supply, fresh from the boiler, producing sudden condensation to the fullest extent. With this brief introduction, I will now pass to the object of my remarks—a triple-expansion engine possessing, I believe, in a marked degree the valuable characteristics found in our latest achievements in engines pertaining to the merchant marine, with the humblest means, fitted to perform the duties of domestic work with the same fidelity and contributory economy as developed in our fastest ocean steamships. This result, however, was not accomplished at once. I had to grope my way, step by step; and it took me over two years of constant study and experimenting, to produce the engine now thrown upon the screen.

In this engine I have succeeded in doing away with all receivers, steam jackets and the valve motion entire. There are no valves, eccentrics, piston, valve, nor eccentric rods, nor any cross-head pins, stuffing boxes, packing rings, nor cut-off gear; and yet the engine is of full piston type, and expands the steam five times. I believe that it possesses a greater degree of simplicity and endurance than even the common slide-valve engine can boast of.

There are three single-acting cylinders, 1, 2, 3, mounted on a crank case. No. 2 is twice the area of No. 1, No. 3 is twice the area of No. 2, and four times the area of No. 1. The pistons perform all the functions of the valves usually employed, passing the steam successively from one cylinder to the other throughout the series, receiving it at ninety pounds pressure, cutting off at two-

thirds stroke in the first cylinder, and exhausting it at about four pounds from the last. In the modern triple-expansion engine, three unequal pistons are used, and three unequal piston valves. Now, please observe the three pistons in this engine. They may be viewed as pistons of the trunk variety, and they are, to all intents and purposes, piston valves as such are made. By coincidence, each occupies the correct relative position as a valve for its neighbor, as nearly as could be desired. The three-throw crank, operated by the pistons having the property of admitting the steam at the commencement of the stroke and cutting off at three-fourths stroke on the piston next following in rotation, all we have to do is to form steam ports leading from cylinder No. 1 to the upper end of cylinder No. 2, and from cylinder No. 2 to cylinder No. 3. The pistons, then, without losing their original identity, become transformed into piston valves governing the operations of the adjacent cylinders. For convenience, a departure is made in the case of the low pressure piston, which is made to carry a piston valve attached to its head, one-half the diameter of the high pressure piston for admitting the steam to the leading or high pressure cylinder. The area presented by this valve being invariably subjected to the pressure of the initial steam, has three advantages: (1.) It tends to equalize the pressure on the pistons by reducing the effective area of the high pressure piston, the more powerful, and assisting the low pressure piston, the less powerful. (2.) It steadies piston 3 on its up-stroke, by providing a certain constant thrust found necessary for running at high speed. (3.) It is further valuable in relieving the engine from dead points, for should the engine stop with the head of the piston valve above the circular port, excluding the steam from the leading cylinder, the first action of the steam admitted by the steam valve will be to push the piston valve down, admitting the steam to the first cylinder. This, in its descent, will pass it to the intermediate cylinder, and from here it will be transmitted to the last of the series, causing the engine to revolve. In any other position, where the circular port is uncovered by the valve, the engine will start, because the steam will then be properly admitted.

Perhaps the simplest way to describe the action of this engine will be to take it just where the pistons are represented; calling the smallest or high pressure cylinder No. 1, the intermediate No.



HENDERSON'S TRIPLE EXPANSION ENGINE.

2, and the largest or low-pressure cylinder No. 3. Piston 1 has already completed its down-stroke, and is ascending; it has just closed the steam port leading into cylinder No. 2, and the steam is thereby cut off from that cylinder. Now it is manifest that unless some provision is made to permit the steam to continue its flow from the one cylinder to the other, the end of further steam expansion has been reached; as the compression above piston 1 would soon stall the engine. But piston 2, at this time, is descending, and has already uncovered an equilibrium passage (in duplicate) leading out of the initial steam port above, opening an avenue for the steam to flow uninterruptedly from above piston 1 into cylinder 2, the piston of which continues to descend by virtue of its greater area (about double that of No. 1). Piston 2 is on the point of admitting steam to piston 3, which has just arrived at the top of its stroke. Such is the present condition of affairs. Now follow out the cycle. Piston 2 will reach its lowest point, and return to the position it now occupies again, at which time piston 3 will have descended three-fourths of its down-stroke; and the same thing will happen, as to passing the steam through the second equilibrium port, opening into cylinder 3, as explained for cylinder No. 2. Just at this point, piston 1 has arrived at the top of its stroke, and the small piston valve carried by piston 3 will give steam to it by the circular port in the cylinder head, the steam entering cylinder No. 1 by the horizontal initial steam port.

When piston 3 descends to within a short distance of the end of its stroke, the exhaust takes place with a fair lead. The circular groove formed on piston 2 is timed to coincide with the lower port opening into cylinder No. 2, to receive the exhaust and pass it across to the exhaust pipe, situated on the back of cylinder No. 2.

We are not quite done with that exhaust yet. Its last effort is made to free the crank case from heated vapors, and maintain the fluid within the case at a practically constant level. A metallic gauge tube is connected to the top and bottom of the crank case, a dip-pipe passes through a stuffing box at the top of the gauge tube, and descends to a point where it is decided to establish the fluid level within the case. A small ejector nozzle is attached to the upper end of the dip-pipe opening into the direct current of the exhaust. It is clear, should the fluid rise and seal the lower end of the dip-pipe, any excess of fluid will be thrown out with the

exhaust. At all other times the outward blast of the exhaust will free the crank case from heated vapors and keep it in a cool condition.

Having now given a brief outline of the manner in which this engine works, I must, for the benefit of those who are critically inclined, explain some of the novel points introduced whereby I bring about the advanced results.

First.—We have the balanced triangular crank shaft, the pin centres of which are 120° apart. It is truly balanced; because the centre of motion coincides with the centre of gravity, just as a disc-crank would be; but it possesses the double advantage of being lighter and more readily got into working position.

Second.—At the bottom of each trunk piston a cover plate is attached, slotted to allow only the vibration of the connecting rod. The object of this is to prevent that splashing of the contents of the case into the interior of the hollow pistons, to lubricate the cross-head pins, which occurs in all engines of this class; converting each hollow piston into a species of *surface condenser*, not only wasting the working steam by unnecessary condensation, but also unduly heating the lubricant, and thereby the journals. You can not bear your hand upon the crank case of any engine now built. It will seem strange to have a high speed engine running with cool bearings and a cold crank case.

Third.—I would briefly draw attention to these hollow heads with removable sleeves, affording constant lubrication of the shaft journals entirely from within the case; and the continuous lubrication of the upper joints of connecting rods from above, by steam condensed there, mixed with whatever lubricant is used on the pistons. The ball and socket joint is worthy of a little consideration. We all know how difficult it is to secure a cross-head pin in that situation, precisely parallel to the driving crank pin. No binding here!

Fourth.—The direct delivery of the steam from one cylinder to the other throughout the series, by the movement of the pistons themselves without valves or receivers, is the dominant feature of the whole.

There remains yet one little thing to describe, without which this engine is of small account. You will notice there are several ports piercing the bore of the cylinders. Take this one, for

instance; say that it is ½ inch wide by 2 inches in length, representing an area of one square inch. Suppose the steam to be 100 pounds pressure, we should have 100 pounds of force jamming the piston over to the other side of the cylinder, producing, in the first case, excessive friction, and in the second, opening an avenue for the steam to escape past the piston. To overcome all this, I simply cast a shallow impression of each port opposite to it, and lead thereto a small semi-circular duct. Now equilibrium of pressure is restored, and the piston is freed from all side thrust.

The form of connecting rod is such as to permit the taking up of any lost motion; the same can be done in regard to the sleeves on the journals. But, it will be observed, the thrust is always in a downward direction, and that even with loose boxes the engine will not rattle.

In regard to the provisions to insure the down thrust, piston No. 1 has always the back pressure from cylinder No. 2 on it. In cylinder No. 2, the piston near the end of its up-stroke passes over the mouth of the second equilibrium passage leading to cylinder No. 3, and cushions on vapor entrapped there for the purpose of compression. In cylinder No. 3, we have the constant pressure of the initial steam upon the area of the piston valve.

All the pistons and piston valve are very long, being twice the length of the stroke, and are provided with water-groove packing, which I prefer in this construction of engine to any other. But any kind of piston packing can be used.

There are many other points I could enlarge upon, but time will not permit. I will, therefore, conclude, praying you to remember that "the greatest discoveries ever made in the world have been made by putting this and that together."

DISCUSSION.

Mr. Fullerton.—"What is the nature of the end boxes on the crank shaft?"

Mr. Henderson.—"They are removable sleeves of phosphor-bronze, pressed into the cast-iron heads. The bearing is tapering one-sixteenth inch to the inch, wear may be taken up by the use of liners following up the taper."

MR. FULLERTON.—"How are they oiled?"

MR. HENDERSON.—"The crank case contains the lubricant. The boss on the cast-iron head is swelled, leaving a cavity when the sleeve is in place. Into this space lamp-wick is introduced through holes opening into the crank case. The lubricating fluid is led by other small holes, drilled through the sleeves, directly to the journals. Constant lubrication is thus maintained entirely from within the case. There are no oil-cups outside. The engineer has nothing to do with oiling the bearings, or, indeed, any part of the engine. It takes care of itself."

MR. FULLERTON.—"Have the two bearings between the central and the outer cranks any caps? or are they merely spring bearings?"

MR. HENDERSON.—"Merely spring bearings. There being no upward strain no caps are needed. These bearings are of Babbitt metal, run when the shaft, or a substitute for it, is in position."

MR. CHAMBERS.—"To what pressure is the steam cushioned in cylinder?"

MR. HENDERSON.—"That would vary under different circumstances, but I would say between one-half and two-thirds of the initial pressure."

MR. CHAMBERS.—"I have had engines running with very good results, cushioned up to boiler pressure. I would ask if the balance in the crank shaft balances the cranks only? or whether it balances the cranks, pistons and connecting rods?"

MR. HENDERSON.—"The crank-shaft is balanced. All, except the crank pin; that, together with the weight of the piston and connecting rod, is balanced by counter-weights in the balance wheels; not by placing a counter-weight on the wheel opposite the weight to be balanced, but by coring out of the rim a corresponding mass on the same side as the weight which is virtually the same in effect, and does not interfere with turning the wheel up in a lathe.

MR. CHAMBERS.—"Then you have a standing balance and not a running balance?"

MR. HENDERSON.—"Each piston and connecting parts are balanced by counter-weights in the rims of the balance wheels, halved between the two, allowing for the difference of leverage

between the distance of the crank-pin centres, and the centres of gravity of the weight in the rim from the centre of revolution. The engine is balanced standing, and is also balanced running, as far as the weights of the pistons, etc., are concerned. The influence of the varying steam pressure is not so easily balanced by a fixed weight. But I have found that if the running parts of the engine is balanced, that is near enough for all practical purposes. The momentum due to steam impulse, is thrown into the balance wheels assisting the pistons to make the return stroke. The excess may be taken up by a properly calculated back pressure on each individual piston."

MR. J. H. COOPER.—"Are the cranks 120° apart?"

MR. HENDERSON.—"Yes! They are arranged equi-distant around he circumference of the crank-orbit."

MR. COOPER.—"The upper ends of the connecting rods appear to be jointed to the pistons by spherical bearings. Do these wear well in practice? In some situations such bearings wear very unequally."

MR. HENDERSON.—"The question of wear being simply one of surfacef, if I can give the same wearing area to a ball, that I can to a cross-head pin, the wear will be the same. The ball-joints in these engines have about double the wearing surface, usually given to cross-head pins in engines of this class. When it is considered that both the joint and piston are relieved from all binding strain, the wear is necessarily reduced. How well this kind of a joint wears can be estimated from the fact the Brotherhood three-cylinder engine has been running with this kind of a joint about fourteen years, and they are building them that way yet."

MR. COOPER.—"Is the exhaust steam condensed in this engine, and is vacuum made available in the third cylinder?"

MR. HENDERSON.—"The steam is not condensed, nor is a vacuum available, I think, in the third cylinder. To do this, I should have to go back to the triple-expansion engine as now built. All has been successfully accomplished. My engine is a departure from it, dropping the condenser, the receiver, the valve motion, and all the complication there existing. It is intended for domestic purposes, where from six to about 100 horse-powers are wanted."

Mr. Cooper.—"Is that an ejector in the exhaust pipe, with pipe dropping into the shaft cistern, and what purpose does it serve?"

Mr. Henderson.—"That is a small ejector operated by the final effort of the exhausting steam. Should the fluid rise in the crank case above where it is wanted, it will also rise in the dip pipe well, and seal the mouth of the dip-pipe, and be thrown out with the exhaust. At all other times the blast of the exhaust across the ejector will free the crank case from heated vapors and maintain it in a cool condition. Up to this time all the crank-case engines are running with cases heated up above 212°. The crank case in this engine will be cold."

Mr. Cooper.—"Allow me to thank Mr. Henderson for the care he has taken in the preparation of the paper and drawings illustrating his new engine, and for presenting them so clearly for the benefit of members of the Institute."

LOSS OF HEAD IN HYDRAULICS.

By Irving P. Church, C.E.

It is the purpose of the present article to offer some comment on Mr. Frizell's paper, in the October number, 1886, entitled "Coefficient of Efflux from an Orifice Furnished with a Short Pipe," in which there seem to be evidences of misconception, or at least a very unusual view, of a fundamental point in hydraulics, viz., "loss of head." It is true that the final result in Mr. Frizell's paper is correct, but the error as to "loss of head" in the first part of his work, is annulled by the subsequent introduction of what, from his standpoint, must be regarded as a "gain of head," an idea which, in the case under treatment, would controvert the principle of the conservation of energy.

Mr. Frizell seems to proceed on the supposition that a loss of velocity $\left(\text{and, therefore, of velocity head } \frac{v^2}{2g},\right)$ in the steady flow of water, implies a "loss of head" in the technical sense (*i. e.*, implying a corresponding loss of energy). It is worth while inquiring, then, what constitutes a "loss of head" in hydraulics, in the sense which implies a loss of energy. That there may be a

loss of velocity without any "loss of head," is immediately apparent when we examine that fundamental formula of hydraulics, viz., Bernoulli's theorem, for the steady flow of water without friction in (motionless) rigid vessels and pipes, as demonstrated and illustrated in all text books on hydraulics (see also Eq. (1) and context in Section 26 of the article, "Hydro-mechanics," *Encyclopædia Brittanica.*) This theorem may be thus written:

$$\frac{v_m^2}{2g} + \frac{p_m}{\gamma} + z_m = \frac{v_n^2}{2g} + \frac{p_n}{\gamma} + z_n \qquad (B)$$

in which m and n are any two positions (fixed in space) in the same stream line, while v_m and v_n denote the velocities of the water passing these respective points; p_m and p_n the internal pressure at m and n, and z_m and z_n the respective heights of m and n from an arbitrary horizontal datum plane situated below both m and n. γ denotes the weight of a unit of volume of water, and g the acceleration of gravity. Naming the terms in Eq. (B), each one of which is in quality a height, or "head," we may say, therefore, that in steady flow without friction, *the sum of the velocity head, pressure head and potential head* at any position (m) in a stream line, is equal to the sum of the corresponding heads at any other position (n) of the same stream line.

If each term in Eq. (B) is multiplied by W, the weight of water flowing per unit-time, it becomes an equation of energy, announcing that without friction the energy present at m, viz., the sum of the three forms, *kinetic energy, pressure energy,* and potential energy, is equal to the corresponding sum at n. [Rankine, p. 100, *Steam Engine,* uses the term actual energy for kinetic, and potential energy for the sum of what are here called pressure and potential energy.] It is, therefore, quite evident that if m and n are at the same level, and the kinetic energy at n is greater than that at n by a certain amount, then the pressure energy at m exceeds the pressure energy at n by an equal amount. (See p. 467, of "Hydro-mechanics," *Ency. Britt., Figs. 33* and *34*, for experiments by Mr. Froude, illustrating this fact.)

If, however, friction cannot be avoided (either *external,* against the vessel walls, or *internal,* through eddying, cross-currents, etc.), or if a motor of proper construction be interposed between m and n, m being down-stream from n, we find that in steady flow the

sum of the three heads at m is always less than that at n, and the deficiency is properly called a "loss of head" (Rankine) or "height of resistance" (Weisbach), and the product of this loss of head by the weight of flow per unit-time gives the energy spent by the water in friction (uselessly) or upon the motor (usefully) per unit-time (*i. e.*, the power). In the case of friction, or internal disturbance, loss of head is peculiarly well-named (as the lost energy cannot be regained in any manner), and is found to be so nearly proportional to the square of the velocity and independent of the pressure, that it is usually expressed in the form

$$\zeta \frac{v^2}{2g},$$

where ζ is a coefficient, found by experiment, and is approximately constant for a given fitting, such as an elbow, bend, short section of pipe having a sudden enlargement, etc., and v is the velocity where the disturbance occurs (or just down stream from it, if there is a change of section). This velocity may be an unknown quantity in some problems, but can usually be expressed in terms of the velocity at the discharging end of pipe.

(The only objects of introducing the $2g$ are that then, $v^2 \div 2g$ being a height, ζ must be an abstract number, and hence the same for any system of units, while $1 \div 2g$ occurs as a factor in other terms, rendering algebraic reduction somewhat simpler. Perhaps this will be satisfactory to M. Cazin. See article by him in *Van Nostrand's Mag.*, for Sept., '86.)

Hence, we may write Bernoulli's theorem, with friction, as follows (m being the down-stream position):

$$\frac{v_m^2}{2g} + \frac{p_m}{\gamma} + z_m = \frac{v_n^2}{2g} + \frac{p_n}{\gamma} + z_n - \Sigma_n^m \zeta \frac{v^2}{2g} \qquad \text{(BF)}$$

(As is well known, for a long pipe

$$\zeta = 4f\frac{l}{d},$$

where f ranges from ·0050 to ·0100, or more, and l = length and d = diameter of pipe.)

Let us apply eq. (BF) to the case of flow into the air through a circular "orifice in thin plate" in the vertical side of a large tank. Let h = vertical height of still surface (exposed to air) above centre

of orifice. Call the (practically) still surface position n, and take position m in the contracted section of jet where the filaments are parallel and consequently under atmospheric pressure. Also, let the datum plane pass through m. Then we must substitute

$$v_n = 0, \frac{p_n}{\gamma} = \text{height of water barometer} = b, z_n = h;$$

$$\frac{p_m}{\gamma} = b, z_m = 0,$$

while from experiment v_m has been found to be

$$= \phi \sqrt{2 g h}$$

(ϕ being the "coefficient of velocity" for this case), so that

$$v_m^2 = \phi^2 \, 2 g h.$$

It is required to find ζ for the loss of head

$$\zeta \frac{v_m^2}{2 g},$$

due to friction around the edges of orifice and contiguous portions of vessel wall. Hence, we obtain

$$\phi^2 h + b + 0 = 0 + b + h - \zeta \phi^2 h \qquad (1)$$

whence

$$\zeta = \frac{1}{\phi^2} - 1 \qquad (2)$$

Again, *Fig. 1*, let us consider a horizontal short pipe in the vertical side of the large tank, the case treated by Mr. Frizell (p. 287, JOURNAL OF THE FRANKLIN INSTITUTE, Oct., '86).

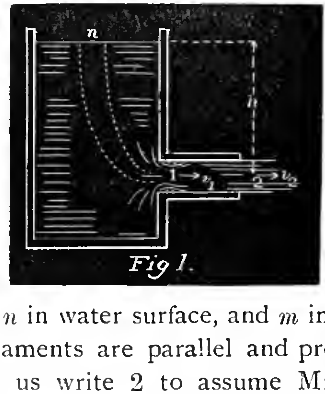


Fig 1.

As before, take n in water surface, and m in the jet at outlet of pipe, where the filaments are parallel and pressure atmospheric; for m, however, let us write 2 to assume Mr. Frizell's notation. Call the contracted section 1 and denote by C the ratio which its

sectional area F_1 bears to that of the pipe F_2; *i. e.*, $F_1 = C F_2$, C being a "coefficient of contraction." Hence, $v_1 = v_2 \div C$. The loss of head between n and 1 is

$$\zeta \frac{v_1^2}{2g},$$

(where ζ has probably the same value as just found in eq. (2), though in the present case the pressure at 1 is much less than atmospheric,) and that lost between 1 and 2 (the *only* loss, begging Mr. Frizell's pardon, if we neglect the slight skin friction in pipe) is

$$\frac{(v_1 - v_2)^2}{2g}$$

by Borda's formula, for a sudden enlargement, *i. e.*,

$$\left(\frac{1}{C} - 1\right)^2 \frac{v_2^2}{2g}.$$

With these two losses of head, then, equation (BF) gives

$$\frac{v_2^2}{2g} + b + 0 = 0 + b + h - \zeta \frac{1}{C^2} \frac{v_2^2}{2g} - \left(\frac{1}{C} - 1\right)^2 \frac{v_2^2}{2g} \quad (3)$$

From (3), after substituting for ζ from eq. (2), we have

$$v_2 = \frac{1}{\sqrt{1 + \frac{1}{(\phi C)^2} - \frac{1}{C^2} + \left(\frac{1}{C} - 1\right)^2}} \sqrt{2gh} \quad (4)$$

If now we assume that C is equal to the coefficient of contraction for discharge through the circular orifice in thin plate into the air, then $\phi C = \mu =$ the coefficient of efflux for that orifice and eq. (4) reduces to Mr. Frizell's result. If with Prof. Unwin (*Encyc. Britann.*) we disregard the loss of head between n and 1, (4) reduces to

$$v_2 = \frac{1}{\sqrt{1 + \left(\frac{1}{C} - 1\right)^2}} \sqrt{2gh} \quad (5)$$

which is Prof. Unwin's form.

(Mr. Frizell has not quoted Prof. Unwin with strict accuracy, having replaced the latter's coefficient of contraction by μ, the coefficient of efflux for the thin-plate orifice.)

Since the only difference between Prof. Unwin's and Mr.

Frizell's treatment of this case (as far as the final result is concerned) consists in the consideration, by the latter, of the entrance friction, which the former does not introduce into his analysis, though he refers to it in the context; and since this friction is not a matter "of impact and momentum," like the resistance due to sudden enlargement further on, it is difficult to see that Mr. Frizell has presented the matter in any new light, except in advancing the novel idea that loss of head (or loss of energy as he calls it) depends only on loss of velocity, but that this idea is erroneous is made evident not only by examining Bernoulli's theorem, as already noted, but also in a striking manner by working a simple numerical example as follows: Let $h = 16$ feet in *Fig. 1*, then we compute v_2 to be $\cdot 815 \sqrt{2gh} = 26\cdot 16$ feet per second. Taking 2 as datum plane, we see that the total head at n is composed of a velocity head of zero, a pressure-head of 34 feet, and a potential head of 16 feet; while at 2 we have a velocity-head of $(26\cdot 16)^2 \div 2g = 10\cdot 62$ feet, a pressure head of 34 feet, and a potential head of zero. The total loss of head then is $(0 + 34 + 16) - (10\cdot 62 + 34 + 0) = 5\cdot 38$ feet. In other words, each pound of water flowing has lost 5·38 foot-pounds of energy between n and 2, and no more, and this loss is mainly due to the sudden enlargement between 1 and 2.

But Mr. Frizell asserts that between positions 1 and 2 alone a loss of head has occurred of an amount

$$= \frac{v_1^2}{2g} - \frac{v_2^2}{2g} = \frac{v_2^2}{2g}\left(\frac{1}{C^2} - 1\right),$$

which if C be taken equal to 0·64 reduces to 15·30 feet, *almost the entire* 16 *feet head* ($= h$) *under which efflux takes place.*

Aside from the facts already referred to on p. 467 of "Hydromechanics," *Encyc. Britann.*, a simple experiment with a common flexible rubber tube will convince any one that the loss of velocity occurring between the narrow and full section of a *gradual* enlargement, when the difference of sectional areas is not extreme, does not imply a corresponding loss of head, the loss of kinetic energy being made up by an equal gain of pressure energy. Suppose the tube attached at one end to a faucet and a jet at about 45° with the horizontal to be issuing from the other extremity, the flow steady. It will now be observed that a very considerable reduction of sec-

tion may be effected at any part of the tube (not so near the jet as to affect the area of its section) by compressing it between the thumb and finger, without appreciably affecting the range of the jet. [See also p. 885, Coxe's *Weisbach.*]

Another instance in point is the "Whirlpool Chamber," invented by Prof. James Thomson, and forming a special feature in the centrifugal pumps manufactured by Williamson Brothers, Kendal, Eng.

This chamber surrounding the centrifugal pump, by furnishing a gradual enlargement of passage way, gives opportunity for the kinetic energy stored in the water, where it leaves the pump blades with considerable velocity, to be gradually transformed into pressure energy, instead of being wasted in internal friction (*i. e.*, eddying), as with other kinds of centrifugal pumps.

As practical examples of the utilization of water-power in each of the three forms just mentioned, it is hardly necessary, perhaps, to call to mind the following familiar and well-marked cases:

(1.) The *vertical water wheel*, using energy in the potential or gravity form; *i. e.*, in passing through the wheel the water experiences no notable change, either in pressure or velocity, but simply in vertical position.

(2.) The *water-pressure engine*, which receives the liquid under pressure and rejects it under a considerably smaller pressure (less than atmospheric if a "draft-tube" is employed), no considerable change in velocity or vertical positions having occurred; *i. e.*, the motor uses the power in the pressure form.

(3.) The turbine of free deviation (and the Pelton "Hurdy Gurdy"), where the water has the same pressure and same vertical position at exit as at entrance, while its absolute velocity has been greatly reduced; *i. e.*, energy has been utilized in the kinetic form.

Cornell University, April 4, 1887.

REPORT ON THE CHEMICAL COMPOSITION OF NATURAL GAS.

Made by PROF. FRANCIS C. PHILLIPS, of the Western University, Allegheny, for the Geological Survey of Pennsylvania.

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(*Concluded from vol. cxxiv, page 256.*)

The following experiments were tried at the valve house of the Philadelphia Company, in the rear of the office building, on Penn Street, Pittsburgh, beginning on March 22, 1887. A Woulfe's bottle containing 200 c. c. purified water, and a second bottle containing cuprous chloride, were connected with a gas meter, and gas allowed to stream slowly through them until 190 cubic feet had passed. The gas thus used comes directly from the Murrysville field. The gas was passed very slowly, so that three days were occupied in the transmission of the volume above named.

The water was then tested for ammonia by Nessler's reagent. No trace could be detected, although, as is well known, this reagent is capable of detecting $\frac{1}{200,000,000}$ part of ammonia in water, with great certainty.

The cuprous chloride was tested for both olefines and carbon monoxide by the method I have detailed, but no trace could be detected of either.

The composition of methane gas by weight is—

Carbon,	74·97	per cent.
Hydrogen,	25·03	"
	100·00	"

Hence this well produces gas approximating in composition to pure methane, and in this respect differs from all those from which samples have been taken. It may be here stated that at the time the sample was collected there was every reason to believe that the gas came exclusively from this one well.

No. 7, Raccoon Creek District. The sample was taken May 2, 1887, from the high pressure main of the Bridgewater Natural Gas

Company, at Rochester, Pa. The pressure at the time was sixty-seven pounds.

The gas is produced wholly from one sand, which is about 1,200 feet below the surface, on Raccoon Creek, in Beaver County. The Bridgewater Company owns twenty-three wells, and supplies the towns of Beaver Falls, Rochester, New Brighton, Phillipsburg, Van Port, Bridgewater, New Sheffield and Shannopin.

The Youngstown Company owns twelve wells in the same region. The gas is almost odorless and the wells produce little or no salt water and no oil.

On causing the gas to bubble through lime water for twenty minutes the fluid remained perfectly clear. After forty minutes a rapid stream of gas caused the lime water to become faintly milky, as seen in a bright light. The proportion of carbon dioxide was far too small to allow of an accurate eudiometric determination. The oxygen reaction was faint but decided.

This gas, on being passed for one hour into a nitrate of silver solution, produced a faint but decided reaction, indicating a trace of sulphuretted hydrogen.

In the statement below, the result of the carbon dioxide test at the main is given.

Determination of—	(1)	(2)	*Mean.*
Nitrogen, ...	10·00	9·82	9·91 per cent.

RESULTS OF ANALYSIS OF RACCOON CREEK GAS.

Nitrogen,	9·91
Hydrogen,	0·
Carbon dioxide,	trace.
Carbon monoxide,	0·
Olefines,	0·
Ammonia,	0·
Oxygen,	trace.
Sulphuretted hydrogen,	trace.
Paraffins,	90·09
	100·00

In a combustion of Raccoon Creek gas, 325·48 cubic centimetres yielded—

H^2O — 0·5108 gm.,	corresponding to H, — 0·05688 gm.	= 23·60	per cent.
CO^2 — 0·6755 gm.,	" to C, — 0·18422 gm.	= 76·40	"
		100·00	"

Hence the paraffins in this gas contain per litre—

0·62827 gm. carbon.
0·19398 gm. hydrogen.

In a second combustion, 398·08 cubic centimetres gas yielded—

H_2O — 0·6254 gm.,	corresponding to H, — 0·06964 gm. =	23·56	per cent.
CO_2 — 0·8286 gm.,	" to C, — 0·22598 gm. =	76·44	"
		100·00	"

Hence the paraffins contain per litre—

0·63010 gm. carbon.
0·19418 gm. hydrogen.

The means of these two results are per litre paraffins—

0·62918 gm. carbon, =	76·42	per cent.
0·19408 gm. hydrogen, =	23·58	"
	100·00	"

This is the only gas which contains traces of sulphuretted hydrogen among those I have examined.

No. 8, Baden. Six miles southeast from Rochester, on the Pittsburgh, Fort Wayne and Chicago Railroad, Beaver County. The samples were taken May 18, 1887, from the Bryan well No. 2, one of four wells belonging to the Baden Gas Company. This gas is produced wholly from one sand, which is 1,396 feet deep, or about 1,300 feet below the Ohio River. This well was drilled in May, 1886.

The Baden wells are on the same anticlinal axis as the Raccoon Creek wells. This same axis continues northward a few miles east of the Speechley wells, near Oil City.

The gas exhibits a decided carbon dioxide and also an oxygen reaction.

Determinations of—	(1)	(2)	*Mean.*	
Nitrogen, . . .	12·26	12·38	12·32	per cent.
Carbon dioxide, .	0·41	0·41	0·41	"

RESULTS OF ANALYSIS OF BADEN GAS.

Nitrogen,	12·32
Carbon dioxide,	0·41
Oxygen,	trace.
Hydrogen,	0·
Carbon monoxide,	0·
Olefines,	0·
Ammonia,	0·
Paraffins,	87·27
	100·00

317·17 cubic centimetres of Baden gas yield on combustion—

H^2O — 0·4892 gm.,	corresponding to H, —	0·05447 gm.	= 23·48	per cent.
CO^2 — 0·6510 gm.,	" to C, —	0·17754 gm.	= 76·52	"
			100·00	"

Hence the paraffins of Baden gas contain per litre—

0·64142 gm. carbon.
0·19681 gm. hydrogen.

In a second combustion, 332·70 cubic centimetres yield—

H^2O — 0·5130 gm.,	corresponding to H, —	0·057127 gm.	= 23·56	per cent.
CO^2 — 0·6843 gm.,	" to C, —	0·18663 gm.	= 76·44	"
			100·00	"

Hence the paraffins contain per litre—

0·64276 gm. carbon.
0·19674 gm. hydrogen.

The means of these two results are per litre of paraffins—

0·64209 gm. carbon,	= 76·48	per cent.
0·19677 gm. hydrogen.	= 23·52	"
	100·00	"

No. 9, Houston well, Houston Station, two miles south of Canonsburg, on the Pittsburgh, Cincinnati and St. Louis Railroad, Washington County.

This well is situated one-third mile west of the Station, on Plum Run. It is drilled nearly through the Gantz sand, and is 1,794 feet deep. An upper gas-producing sand is found at 850 feet, but this is cased off, so that the well may be considered to yield gas from the Gantz sand exclusively.

The gas from the upper sand is said by well superintendents to burn with a whiter but more sooty flame than that from the greater depth. According to the statements generally heard at the wells, the occurrence of an upper, less productive gas-sand yielding gas of greater illuminating power, is a very common feature in many gas wells. The sample was collected on March 18, 1887.

The gas exhibits an oxygen reaction and causes a rapid precipitation in lime water.

Determinations of—	(1)	(2)	*Mean.*
Nitrogen, ...	15·23	15·37	15·30 per cent.
Carbon dioxide, .	0·42	0·46	0·44 "

RESULTS OF ANALYSIS OF HOUSTON GAS.

Nitrogen,	15·30
Carbon dioxide	0·44
Oxygen,	trace.
Olefines,	0·
Carbon monoxide,	0·
Ammonia,	trace.
Hydrogen,	0·
Paraffins,	84·26
	100·00

310·20 cubic centimetres of Houston gas yielded on combustion—

H^2O — 0·4601 gm., corresponding to H, — 0·05124 gm. = 23·20 per cent.
CO^2 — 0·6217 gm., " to C, — 0·16955 gm. = 76·80 "

100·00 "

Hence the paraffins contain per litre—

0·64871 gm. carbon.
0·19602 gm. hydrogen.

In a second combustion, 293·35 cubic centimetres yielded—

H^2O — 0·4392 gm., corresponding to H, — 0·04891 gm. = 23·44 per cent.
CO^2 — 0·5855 gm., " to C, — 0·15968 gm. = 76·56 "

100·00 "

Hence the paraffins contain per litre—

0·64604 gm. carbon.
0·19786 gm. hydrogen.

The means of these two analyses are per litre of paraffins—

0·64737 gm. carbon, = 76·68 per cent.
0·19694 gm. hydrogen, = 23·32 "

100·00 "

The analyses above detailed were carried out with great care, and every known precaution observed in order to secure accuracy. The results represent the character of the gas from particular wells or group of wells, scattered over a large region, and as it flowed from the wells on a single day.

It is questionable whether they can be considered to represent the average composition of natural gas, for the reason that the gas territory is so vast in extent. According to the above results natural gas is not so complex a substance as has been heretofore supposed.

The samples examined may be said to consist simply of the hydrocarbons of the paraffin series, among which methane predominates. It is to these bodies that the fuel value of the gas is due. Inasmuch as most of the gas conveyed through pipe lines deposits little or no liquid hydrocarbons, it is evident that the higher paraffins are not present in notable quantity.

The method I have used in testing for the hydrocarbons of the olefine series enables me to state with much confidence that these bodies—ethylene, propylene, butylene, etc—are absent. Hydrogen I have found in Speechley gas alone, although the utmost care has been taken in the examination.

Perhaps still smaller quantities may have escaped detection in other gas samples.

Sulphuretted hydrogen was found only in Raccoon Creek gas, but in faint traces. Oxygen is present in all, but in such small quantities that I have never succeeded in accurately determining its real percentage.

As nearly as I can estimate, the Wilcox contains more oxygen than any other, and Murrysville the least. Ammonia was found, in traces only, in Houston gas. Carbon monoxide was not found in any of the samples.

A comparison of the results in the accompanying table shows that the different gas samples differ mainly in the following particulars:

(1.) The proportion of carbon to hydrogen in the contained paraffins; that is to say, the ratio of the lower to the higher paraffins. Fredonia is seen to be the richest gas in carbon.

(2.) The proportion of nitrogen, which varies between 2·02 and 15·30 per cent. The three gas fields, Speechley, Baden and Raccoon Creek, approximately on the same anticlinal (according to Mr. I. C. White), produce gas having very different quantities of nitrogen.

The resemblance between the Fredonia, Sheffield, Kane, Wilcox and Raccoon Creek gas, as regards the proportion of nitrogen, is a matter of interest, although not explainable.

In the case of Murrysville, Speechley and Fredonia gas, the density, richness in carbon and calorific power of the contained paraffins are inversely as the proportion of nitrogen. It is a curious fact that there is a certain continuity as regards composi-

tion in the case of the Fredonia, Kane, Sheffield and Wilcox gases, which disappears on reaching the Speechley field. In proceeding southward south of Speechley, much greater differences occur.

TABLE I.

CONSTITUENTS.	Fredonia.	Sheffield.	Kane.	Wilcox.	Speechley.	Lyon's Run near Murrysville.	Raccoon Creek	Baden.	Houston.
Nitrogen, ...	9·54	9·06	9·79	9·41	4·51	2·02	9·91	12·32	15·30
Carbon dioxide, ...	0·41	0·30	0·20	0·21	0·05	0·28	trace.	0·41	0·44
Hydrogen, ...	0·	0·	0·	0·	0·02	0·	0·	0·	0·
Ammonia, ...	—	0·	0·	0·	0·	0·	0·	0·	trace.
Oxygen, ...	trace.	trace.	trace.	trace.	trace.	trace.	trace.	trace.	trace.
Sulphuretted hydrogen, .	—	0·	0·	0·	0·	0·	trace.	0·	0·
Paraffins, ...	90·05	90·64	90·01	90·38	95·42	97·70	90·09	87·27	84·26
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00

The paraffins contained in these gas samples have the following composition *by weight:*

Carbon, ...	78·14	76·69	76·77	76·52	77·11	74·96	76·42	76·48	76·68
Hydrogen, ...	21·86	23·31	23·23	23·48	22·89	25·04	23·58	23·52	23·32
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00

(3.) The carbon dioxide, which varies within very narrow limits The only gas in which it disappears is that from Raccoon Creek, although Speechley gas contains barely more than at race.

At Oil City, a sand is found 582 feet below low water mark in the Allegheny River, which produces gas of low pressure, amounting, it is said, to twenty pounds when shut in for some time. This gas is used in the Oil Well Supply Company's works for heating purposes.

It bears the same relation to the Speechley gas-sand, 1,900 feet deep, as the shallow gas sands usually to the deeper and more productive sand rocks.

A determination of the nitrogen in the gas from this upper rock gave 5·62 per cent. Speechley gas contains 4·51 per cent. The sample was collected on April 13th, the day on which the Speechley samples were taken.

The Speechley gas wells are six miles distant from this well. Tests for hydrogen, olefines, carbon monoxide, and dioxide and ammonia in this gas all led to negative results.*

In conclusion, I have to express my indebtedness for information and for facilities in conducting tests and examinations at the wells, to the following gentlemen: Mr. K. Chickering, of the Oil Well Supply Company, Oil City; Mr. W. C. Henry, of the United Natural Gas Company, Wilcox; Mr. Walter Horton and Mr. John McNair, of Sheffield; Mr. J. D. Bruder, of Kane; Mr. E. J. Crissey, of Fredonia; Mr. S. F. Gayley, of Rochester; and to the officers of the Philadelphia Gas Company, the Baden Gas Company, and the Pennsylvania Gas Company, of Pittsburgh.

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* For the information of those who are not familiar with the chemistry of the subject, it may be of importance to call attention to the two very different modes of occurrence of hydrogen in fuel gases.

As *free* or *uncombined* hydrogen, it is often reported to occur in considerable quantity in natural gas, and constitutes often from thirty to forty per cent. of ordinary coal gas.

In this form of *free* hydrogen, it may be separated and removed more or less completely from the other gaseous constituents by exposure of the gas to a porous wall of plaster, through which the extremely light, *free* hydrogen more readily passes. In the above detailed analyses, *free* hydrogen was found in a single instance, and in traces only (Speechley gas).

In chemical union with carbon, hydrogen constitutes about one-fourth part by weight of all the natural gas now used as fuel in Pennsylvania. In this form of occurrence, as a hydrocarbon, hydrogen cannot be separated from the other constituents by mechanical means. The products of the combustion of all such gas will be the same in kind—carbon dioxide and water—whether the hydrogen be present wholly as a hydrocarbon, or partly as such and partly *free*.

CALCULATION OF THE FUEL VALUE OF NATURAL GAS.

The calorific power of any combustible may be determined by measuring the number of kilogrammes of water heated from 0° to 1° by one kilogramme of the fuel in burning, or by a calculation. The difficulties and inconveniences encountered in the first method necessitate commonly a resort to the second.

Pure charcoal, in burning, produces, according to the researches of Favre and Silbermann (in 1849), 8,080 heat units, or one kilo. in burning will raise the temperature of 8,080 kilos. of water from 0° to 1° C.

By the same authors it was found that one kilo. of hydrogen in burning, generates a quantity of heat sufficient to warm 34,462 kilos. of water from 0° to 1° C.; that is, 34,462 heat units. Later determinations have been made by various authors, the most important being by Thomsen, who found 34,180 (*Berichte der deutschen chemischen Gesellschaft*, 1873, p. 1883), and by Berthelot, who obtained the number 34,600 (*Comptes Rendus*, 1880, p. 1240). The value assigned by Thomsen, viz., 34,180, is probably the more correct.

If it were possible that a fuel should contain pure hydrogen and charcoal, a calculation of its heating power would lead to very correct results. It is found, however, that when a *compound* of carbon and hydrogen is burned, the number of heat units produced will not equal the number obtained when the same quantities of carbon and hydrogen are burned separately.

Thus a kilo. of methane produces 13,270·5 heat units, but if the same quantities of carbon (as charcoal) and hydrogen were burned separately in a calorimeter, 14,613 heat units would result (assuming that the carbon produces 8,080 and the hydrogen 34,180 heat units per kilo. burned).

This difference between the calculated amount of heat, and the actually available heat, 14,613 — 13,270 = 1,343 heat units is 9·19 per cent. of the theoretical yield. For practical applications, this is a loss of heat, which must be considered to represent the quantity of energy required to overcome the mutual affinity of the carbon and hydrogen, which are to be first separated before they are burned to carbon dioxide and water.

With more complex compounds the available heat of combustion does not fall so far short of the theoretical maximum, and it may

be stated in a general way that the greater the number of carbon atoms in the compound the more closely will the available and actual number of heat units coincide. This statement is especially true of certain series of hydrocarbons. The following table will serve to illustrate this in the case of the first three members of the paraffin series. For the higher paraffins no determinations have yet been made:

TABLE II.

SHOWING RATIO OF AVAILABLE TO CALCULATED HEAT OF COMBUSTION IN THE CASE OF CERTAIN HYDROCARBONS.

Name.	Symbol.	Calculated Heat Units, assuming that the Carbon and Hydrogen produce the Maximum of Heat and are burned separately.	Available Heat as determined by Calorimetric Measurement.	Percentage of Available on Theoretical Maximum of Heat Units.
		Per Kilo. of Paraffin.	Per Kilo. of Paraffin.	
Methane,	CH^4	14,613	13,270	90·81
Ethane,	C^2H^6	13,310	12,373	92·95
Propane,	C^3H^8	12,835	12,052	93·89

It has been shown by Thomsen that isomeric hydrocarbons, or those which differ in properties, though having identical composition, may produce different quantities of heat when burned. Thus—

	Symbol.	*Heat Units.*
Propylene,	C^3H^6	11,757
Trimethylene,	C^3H^6	10,917
Difference,		840

The chemical formulæ given show them to have the same composition, and yet these hydrocarbons would be represented by different values, if used as fuels.

The presence of isomers among the hydrocarbons of natural gas would tend to interfere with the correctness of a calculation of its fuel value.

No isomers are known in the case of methane ($C\,H^4$).

Berthelot has stated that a second hydrocarbon isomeric with ethane (C^2H^6) exists, which produces on burning 12,776 heat units, instead of 12,373, the number as determined by Thomsen.

Thomsen's researches have disproved this assertion, however, and have shown conclusively that ethane produced in a variety of ways invariably possesses the same calorific power (*Berichte der deutschen chemischen Gesellschaft*, 1881, p. 500). Isomers of the higher paraffins no doubt occur in gas as well as in petroleum, but when it is considered that in gas the higher paraffins occur only in small quantity, and moreover that the calculated and the available calorific power differ much less in these higher members than in methane and ethane, the danger of error from the presence of such isomers cannot be considered likely to effect the calculated results.

The calorific power of methane was determined by Andrews in 1848, as 13,108 heat units (*Philosophical Magazine*, 1848, p. 321), and by Favre and Silbermann in 1853 as 13,063 heat units.

In 1880, Thomsen assigned it the value 13,345·6, and this number agrees closely with that obtained by Berthelot in the same year, viz., 13,343·8. More recently Thomsen has corrected his former result, and now gives 13,270·5 as the most probable number (Berthelot, *Comptes Rendus*, 1880, p. 1240). Thomsen (*Berichte der deutschen chemischen Gesellschaft*, 1880, p. 959 and 1321, ref., and 1887, p. 77, ref.).

The elaborate researches of Julius Thomsen in thermo-chemistry (*Thermo-chemische Untersuchungen*, Leipzig) have reached the fourth of a series of large volumes, and although designed primarily as a contribution to theoretical chemistry, they supply data likely to prove of great value in the study of fuels for metallurgical and other technical purposes.

The actual calorific power of a gas fuel may now, by the use of such data, be more satisfactorily determined by calculation, provided its composition is known, than by the use of a calorimeter. In this respect, there is an important difference between gas fuels and the various kinds of coal. Coal being a compound of carbon, hydrogen and oxygen, of a highly complex character, or possibly a mixture of such compounds, no such plainly definable relationship exists between the theoretical maximum and the available heat quantity per unit-weight burnt.

The percentage composition by weight of the paraffins likely to occur in natural gas is expressed in the following table. Small quantities of condensable vapor of higher paraffins occur in the gas

in some places, as is evident by the condensation of benzine in pipes. The heavier vapors occur usually in very minute quantity, if at all.

TABLE III.

SHOWING THE COMPOSITION BY WEIGHT OF SOME OF THE LOWER PARAFFINS.

Name.	Symbol.	Per Cent. Carbon.	Per Cent. Hydrogen.
Methane,	CH^4	74·97	25·03
Ethane,	C^2H^6	79·96	20·04
Propane,	C^3H^8	81·78	18·22
Butane,	C^4H^{10}	82·72	17·28
Pentane,	C^5H^{12}	83·29	16·71

The analyses of natural gas above detailed show a variation in the proportion of carbon and hydrogen in the case of the two extremes of 3·18 per cent. Thus, the paraffins of Murrysville gas contain—

Carbon,	74·96	per cent. by weight.
Hydrogen,	25·04	" "
	100·00	" "

and in case of Fredonia gas—

Carbon,	78·14	per cent. by weight.
Hydrogen,	21·86	" "
	100·00	" "

From the tabular statement of the composition of the lower paraffins, it appears that Murrysville gas, as obtained at the Hukill well, has nearly the composition of methane, while, disregarding again the nitrogen and carbon dioxide present, the Fredonia gas, the richest in carbon, approximates in composition to a mixture of equal volumes of methane and ethane, of which the actual composition would be by weight—

Carbon,	78·22	per cent.
Hydrogen,	21·70	"
	100·00	"

By this I do not imply that it actually contains these two paraffins in the proportions named, for it is possible that the gas in question contains more of methane and a very small quantity of some of the higher paraffins, propane, or quartane, etc.

As I have stated in regard to the analyses, the exact determination of the percentage of individual paraffins is a matter of such extreme difficulty that it may be considered practically impossible.

If we assume that Fredonia gas really contains equal volumes of methane and ethane, and calculate its calorific power accordingly, the following error may be committed. The gas may contain a larger amount of methane than was assumed, and consequently a very small quantity of quartane or pentane, *for although the percentage of carbon and hydrogen is definitely fixed by the analysis, it is still a question as to the arrangement of the carbon and hydrogen in the form of higher or lower paraffins.* As the difference between the available and theoretical heat of combustion is greater in the case of methane and less in the higher paraffins, an under-estimate of the quantity of methane would lead to too high a value for the available heat of combustion. On the other hand, an under-estimate of the proportion of the higher paraffins would cause the available heat, as expressed in heat units, to be rated too low, supposing that in both cases the absolute quantities of carbon and hydrogen remained constantly the same.

This error would be small in most instances, but in the extreme case of the two gases, consisting of methane and ethane, respectively, the error from this source would exceed one per centum. I have attempted to correct this error, as will be shown below.

The curious and intimate relationship of the paraffins is well illustrated by the fact that a mixture of one cubic metre each of methane, ethane and propane will contain the same proportions of carbon and hydrogen, and will consequently yield the same quantities on burning of CO^2 and H^2O as three cubic metres of the intermediate hydrocarbon ethane.

1 cubic metre of methane weighs 0·7148 kilos., and generates heat units,	9,485
1 cubic metre of ethane weighs 1·34016 kilos., and generates heat units,	16,582
1 cubic metre of propane weighs 1·9656 kilos., and generates heat units,	23,688
Total,	49,755
3 cubic metres of ethane generate on burning heat units,	49,746
Difference,	9

The numbers expressing the heat produced are obtained by

multiplying the weight of the cubic metre by 13,270, 12,373 and 12,052, respectively, as given in Table II.

The difference is so slight, amounting to only nine heat units, that it is evident that it would have been sufficiently accurate to assume this mixture of three hydrocarbons to consist of the intermediate member, ethane, in so far as the calculation of the fuel value is concerned.

Or it may be more broadly stated that, with a view to the calculation of the calorific power of natural gas, it is sufficiently accurate to assume that the natural gas (containing no hydrocarbon of the olefine series) has the simplest constitution consistent with its percentage by weight of carbon and hydrogen, and then to determine its fuel value accordingly.

Fredonia gas, as shown in the table of analyses, consists of 90·05 per cent. of paraffins, together with 9·54 per cent. of nitrogen and 0·41 per cent. carbon dioxide. The paraffins in this gas consist of 0·80406 kilo. carbon and 0·22494 kilo. hydrogen per cubic metre.

The theoretical maximum of heat units for these paraffins is calculated as follows:

$$\begin{aligned} 0{\cdot}80406 \times 8{,}080 &= 6{,}497 \\ 0{\cdot}22494 \times 34{,}180 &= 7{,}288 \\ \hline &\ 13{,}785 \end{aligned}$$

When CH^4 burns, only 90·81 per cent. of the theoretical heat is available. When C^2H^6 burns, 92·95 per cent. can be utilized.

Hence if Fredonia gas is to be looked upon as a mixture of equal volumes of the two hydrocarbons methane and ethane, it will contain about one and 1·87 parts by weight, respectively, or approximately two parts by weight of ethane and one of methane.

The available heat of combustion can be determined by multiplying the theoretical maximum by a factor which is intermediate between ·9081 and ·9295, and as a very close approximation the fraction

$$\frac{2\ Et. + Mt.}{3 \times 100}$$

will, I think, be sufficiently accurate. In this, *Et.* equals the percentage of available on theoretical maximum heat for ethane, and

Mt. equals the same ratio for methane. Substituting in this fraction

$$\frac{2 \times 0{\cdot}9295 + 0{\cdot}9081}{3} = {\cdot}9224.$$

The theoretical maximum heat of combustion of the Fredonia gas as calculated above, is 13,785 heat units.

Then, 13,785 × 0·9224 = 12,715 as the available heat units due to the paraffins in the gas. As there are 90·05 of paraffins, the remainder consisting of nitrogen and carbon dioxide, the above number will be still further reduced and 12,715 × 0·9005 = 11,450, the available heat produced by one cubic metre of Fredonia gas.

In the case of the gas from Sheffield, Kane, Wilcox, Raccoon Creek, Baden and Houston, there is a general similarity as regards the percentage of carbon and hydrogen. Wilcox gas may be regarded as representing approximately the average, and as a calculation shows that a mixture of four volumes of methane and one volume ethane contains carbon 76·54 and hydrogen 23·46, we may, for the purpose of the present calculation assume that the above-mentioned six gases contain approximately these proportions of the two named paraffins. For such a mixture, the factor by which to obtain the available calorific value, will be

$$\frac{2\,Mt. + Et.}{3 \times 100} = 0{\cdot}9152.$$

This factor has accordingly been used in the case of the above-named gases. Speechley gas may be considered to contain five volumes of methane and two volumes of ethane, for the purpose of the present calculation, and the factor will be

$$\frac{3\,Et. + 4\,Mt.}{7 \times 100} = 0{\cdot}9173.$$

Murrysville gas contains nearly pure methane, and consequently the factor will be ·9081.

It is not implied in the above consideration that the actual proportions of what may be regarded as the most commonly occurring paraffins—CH^4, C^2H^6, C^3H^8, etc., can be accurately stated, for this I believe to be impossible. These proportions have been assumed as not inconsistent with the analytical data, merely for the purpose of obtaining approximately correct values for the factors to be used in the calculation of the calorific power of gas.

The following table, IV, contains the results of the calculation carried out as explained. Column No. 2 in this table expresses the quantities of carbon and hydrogen contained in one cubic metre of each gas. In Column No. 3 are given the factors, the derivation and use of which has already been pointed out.

TABLE IV.

FUEL VALUE OF NATURAL GAS.

	2		3	4	5	6	7
Gas Fields.	Weight in Kilos. of Carbon per Cubic Metre of Paraffins.	Weight in Kilos. of Hydrogen per Cubic Metre of Paraffins.	Factor.	Available Heat Units per Cubic Metre of Gas.	Available Heat Units per 100 feet of Gas.	Pounds of Water at Boiling Point, evaporated by 100 feet of Gas.	Pounds of Pure Charcoal, equal in Heating Effect to 100 feet of Gas.
Fredonia,	0·80406	0·22492	0·9224	11,449	32,421	133·30	8·845
Sheffield,	0·65526	0·19924	0·9152	10,040	28,430	116 89	7·756
Kane,	0·65669	0·19866	0·9152	10,354	29,319	120·54	7·999
Wilcox,	0·64622	0·19828	0·9152	9,925	28,102	115·54	7·667
Speechley,	0·69857	0·20738	0·9173	11,144	31,554	129·73	8 609
Lyon's Run, near Murrysville,	0·53741	0·17950	0·9081	9,296	26,321	108·22	7·181
Raccoon Creek,	0 62918	0·19408	0·9152	9,661	27,355	112·47	7 463
Baden,	0·64209	0·19677	0·9152	9,515	26,941	110·77	7·350
Houston,	0 64737	0·19694	0·9152	9,224	26,119	107·38	7·126

1 cubic metre = 35·3166 cubic feet.

1 kilogramme = 2·20462 pounds avoirdupois.

This factor is a fraction. Its numerator represents the actual number of heat units produced in the burning of the unit-weight of the total paraffins, the number being ascertained from a consideration of the percentage of carbon and hydrogen in the gas. The denominator represents the number of heat units obtained when the quantities of contained carbon and hydrogen are multiplied by the numbers 8,080 and 34,180 respectively, and the products added.

Column No. 4 gives the actual fuel value of each gas expressed in heat units per cubic metre. These numbers represent the heat of combustion calculated for the carbon and hydrogen separately, these two added together, and their sum multiplied by the corresponding factor in No. 3.

The numbers in Column No. 5 indicate kilogrammes of water which can be warmed from 0° to 1° C. when 100 cubic feet of the

respective gas, measured at 0° C., and under a barometric pressure of seventy-six centimetres, are burned at an initial temperature of 18° C., or 64°·4 F. (This last is the temperature assumed by Thomsen in his determinations), and assuming that the products of combustion are liquid water and gaseous carbon dioxide.

In Column No. 6 are stated the numbers of pounds avoirdupois of water which, theoretically, should be boiled away at 100° C. into steam at the same temperature, and under atmospheric pressure, when 100 cubic feet of gas are burned. The latent heat of evaporation of water in this calculation has been assumed as 536·2 heat units (Berthelot, *Comptes Rendus*, 1877, p. 646).

In the seventh column a comparison is given between gas and pure charcoal, assumed free from ash. Charcoal has been chosen rather than coke or coal, for the reason that exact calorimetric data as to the latter fuels are as yet difficult to obtain, and calculated values are uncertain.

An impression prevails, based partly upon analytical data, and partly upon a supposed variation in the steam-producing power, that natural gas is subject to constant fluctuations in composition. To what extent such fluctuations are liable to affect the value of the results of the above calculations, I am wholly unable to state.

In view of these reported changes, it is to be regretted that more abundant data are not at hand upon which to base a conclusion as to the real nature of the fluctuations in composition. If such changes occur, are they progressive or irregular? Are they of such a character as to cast any light upon the question of origin, which every one asks but no one can answer? Are they to be regarded as a factor in determining the durability of a gas well?

DOWNWARD-DRAUGHT FURNACES.

CARLOS A. LOZANO and H. F. T. ERBEN.*

The idea of drawing or forcing the air downward through the burning coal, instead of upward, is not new; but Messrs. Post and Sawyer have made a device, with a grate kept at comparatively low temperature, by the circulation of water in the pipes used as grate bars, which has been the means of making the downward-draught furnace a practicable reality. They designed attachments for application to various kinds of boilers in place of the ordinary furnace, a number of which are said to be in successful operation, and the present device was claimed to be superior to any other boiler furnace, and capable of improving the performance of any boiler whatever.

Having undertaken, for our graduating thesis, an investigation of this new furnace, with a view to find out what advantages, if any, were to be gained by it, specially if any improvement in the combustion or any diminution in the quantity of the air of dilution, we arranged with the agents of the device, and with the engineers, Messrs. Richard H. Buel and E. E. Magovern, who had been engaged to make certain tests, to assist in the tests and take all the data we wanted during them.

The experiments were made upon a return tubular boiler, first with the boiler and ordinary furnace as it originally stood, and then after the Post and Sawyer device had been attached; the firing being conducted by the same man, Mr. J. F. Ayer, an expert fireman, in the employ of Messrs. Post and Sawyer, and the coal used being of one kind, an amount sufficient for the two tests having been stored for the purpose.

Both tests were conducted at the maximum rate of combustion possible under the existing natural draught.

The following is an account of the observations taken, and a few deductions therefrom.

BOILER PLANT.

The boiler plant consisted of a cylindrical tubular boiler, 14 feet long, 5 feet diameter, having 90 tubes, 3 inches external dia-

* Graduation Thesis.—Stevens Institute of Technology.

meter, and a steam drum, diameter 30 inches, height 24 inches, near the top of which drum, or dome, the steam pipes are connected at the front and back; plain brick setting, internal distance between side walls, 5 feet; grate horizontal, 5 x 5 feet, and 22 inches below the shell of the boiler; bridge wall horizontal, 6½ inches below the shell; one-half of the shell exposed to the products of combustion, these passing back under the boiler, and then returning through the tubes into front connection, to be carried off by an iron flue, 20 inches in diameter, this latter discharging into a rectangular iron stack, 3 feet 11 x 7 inches in cross section and 110 feet high. The bottom of the glass gauge was on a level with the top of the upper row of tubes. The upper part of the shell and the steam drum were covered with asbestos. The feed water was admitted into the boiler at the bottom, near the back end, after having passed through a feed-water heater heated with exhaust steam.

DOWNWARD-DRAUGHT ATTACHMENT.

Attached to the border above described was the Post and Sawyer's downward-draught device, as intended for application to externally fired tubular boilers.

The attachment referred to consisted of steel water-leg, 4 feet 6 inches wide, 12 inches deep at top, and 8¾ inches deep at bottom, extending 3 feet below the shell of the boiler; 2 inclined circulating pipes, not in the picture, 4 inches diameter, 56¾ inches long, connecting the sides of the water-leg near the bottom with the sides of the boiler shell near the front; one vertical circulating pipe, 4 inches diameter, 44½ inches long; 21 pipes forming grate-bars, 1½ inches diameter, 54⅜ inches long, set highest at the back, at an inclination of 2 inches to the foot; seven circulating pipes, 3 inches diameter, 55⅝ inches long, highest at the front and having a drop of ¾ inch per foot; seven headers connecting the pipes of the water-grate in groups of 3 with the 3-inch circulating pipes; a branch of the feed pipe, connecting with the water-leg near the bottom, opposite the vertical circulating pipe, so that the feed water divided, part going directly into the boiler at the bottom near the back, as before, and part entering the water-leg first.

DIMENSIONS COMPARED.

Item.	Before Change.	With the Post and Sawyer Attachment.
Grate surface, square feet,	25·	20·
Air space over bridge wall, square feet,	2·89	4·27
Air space in grate bars, square feet,	6·6	4·89
Internal area of boiler tubes, square feet,	3·8	3·8
Area of round flue, square feet,	2·18	2·18
Area of rectangular flue, square feet,	2·29	2·29
Water heating surface, square feet. — Boiler proper,	1127·13	1122·34
Water heating surface, square feet. — Attachment,	. . .	127·29
Water heating surface, square feet. — Total,	1127·13	1249·63
Ratios to grate surface, — Water heating surface,	45·09	62·72
Ratios to grate surface, — Air space in grate bars,	·264	·225
Ratios to grate surface, — Air space over bridge wall,	·116	·214
Ratios to grate surface, — Internal area of tubes,	·152	·190
Ratios to grate surface, — Area of round flue,	·087	·109.
Ratios to grate surface, — Area of rectangular flue,	·091	·115

RECORD OF THE OBSERVATIONS DURING THE FIRST TEST.

May 5–6. 94 Liberty Street, N. Y.

Time.	Steam Gauge pressure lbs per sq. in.	Draft Inches of Water.	Height of Water in glass gauge. Inches.	Coal. Pounds.	Feed. Pounds.	Water Meter Readings. Cubic Feet. (Uncorrected Readings)	Barometer Inches.	Thermometer Attached to the Barometer. F.
10 A. M.	62	·2	3·75	840	2,660	7511·	30·21	89·
11	59	·3	3·625	420	2,660	7542·5	30·22	89·
12 M.	40	·15	2·75	140	1,900	7585·	30·20	88·2
1 P. M.	44	·1	3·75	700	2,280	7628·	30·20	87·
2	41	·1	1·5	. .	1,900	7658·5	30·17	97·
3	41	·1	1·6	420	2,660	7700·5	30·18	86·5
4	39	·1	3·	. .	3,040	7743·	30·16	86·
5	42	·1	1·	420	1,520	7774·	30·17	86·
6	43	·1	2·5	280	3,040	7820·	30·15	87·
7	60	·1	3·5	560	2,660	7864·3	30·18	89·
8	48	·1	2·75	. .	2,660	7906·	30·23	89·
9	65	·1	2·75	280	2,280	7946·	30·21	88·
10	63	·1	3·75	280	2,660	7990·	30·22	88·
11	52	·05	2·4	312	1,900	8018·	. .	. .
12 Mid.	65	·2	4·	. .	3,420	8064·	. .	. .
1 A. M.	. .	. .	. .	. .	380	8079·	. .	. .
	164 / 15 = 50·9	2·1 / 15 = ·14	42 625 / 15 = 2·84	4,652	37,620	605½ × 62·3538 = 37755 Pounds.	392·5 / 13 = 30·2 = 30·05 at 32° F.	1149·7 / 13 = 88°·4

Time.	Temperature Fahrenheit.					
	Feed.		Fire Room.		Flue.	
	Initial.	Final.	Dry Bulb.	Wet Bulb.	Thermometer	Pyrometer. (Uncorrected Readings.)
10 A. M.	. . .	103	108·7	96 3	370	430
11	. . .	145	108·	96·2	350	410
12 M.	. .	197	110·7	95 3	310	380
1 P. M.	. . .	197	112·5	96·	330	400
2	. . .	200	112·	95·5	. .	380
3	. . .	197	110·	96·	. . .	400
4	62 25	200	110·5	97·	. . .	375
5	68·	200	112·	96·	. . .	380
6	63·	200	112·	95·	. . .	400
7	61·	200	108·	97·	. . .	410
8	62·	190	112·	97·	357	410
9	. . .	180	112·	97·	338	408
10	. . .	210	112·	95·	338	410
11	60·	200	106·	95·	348	412
12 Mid.	. . .	200	112·	94·	. . .	400
	$\frac{376·25}{6}$	$\frac{2819}{15}$	$\frac{1658·4}{15}$	$\frac{1438·3}{15}$	$\frac{2741}{8}$	$\frac{6005}{15}$
	= 62·7	= 187·9	= 110·6	= 95 9	= 342·6 Probable mean all the time, 340°.	= 400·3

Remarks.

Hauled fire at 8.30 A. M.

Steam pressure, 65. Water gauge, 4 inches.

Started fresh fire at 8.42 A. M., with 323·5 pounds of wood, equivalent to ·4 × 323·5 = 129·4 pounds coal.

Steam pressure, 60. Water gauge, 4 inches.

Trial ended at 12.42 A. M.

Steam pressure, 60. Water gauge, 4 inches.

Commenced to feed at 8.47 A. M.

Put on first coal at 8.49 A. M.

Fired 23 times during the test.

Sliced the fire 8 times.

Cleaned the fire once.

Contents of the furnace and ash pit weighed at the end of the test,	1,261 pounds.
Picked out unburned coal,	713 "
Ashes,	548 "

Meter at commencement of the test,	7473·5
Meter at end of the test,	8079·
Cubic feet,	605·5

Made two tests of furnace temperature, heating 1·6824 kilogrammes of wrought iron in the furnace, and putting it into 100 pounds water, raising the temperature 10° in each case.

	Pounds.
Coal fed,	4,652
Wood equivalent,	129·4
	4,781·4
Picked from ashes,	713
Coal burned,	4,068·4
Moisture in the coal, 4781·4 × ·0385,	184·1
Dry coal burned,	3,884·3
Ashes,	548·
Combustible burned,	3,336·3

The following is Mr. E. E. Magovern's record of his calorimeter experiments:

Number of Test.	Weight of Water in Calorimeter. Pounds.		Temperature of Water. F. Degrees.		Steam Pressure Pounds per Square Inch.	Duration of Test. Minutes.
	Initial.	Final.	Initial.	Final.		
1	305·	339·	62·3	164·	39	5·033
2	304·	314·37	68·	101·	40	1·5
3	304·1	318·6	63·	110·	55	2·033
4	304·2	343·7	61·	178·	60	5·038
5	304·5	329·12	62·	146·	60	3·046
6	304·3	324·7	60·	131·	60	2·55

RECORD OF THE OBSERVATIONS DURING THE SECOND TEST.

June 21–22. 94 Liberty Street, N. Y.

Time.	Steam Gauge pressure lbs per sq. in.	Draft Inches of Water.	Height of Water in glass gauge. Inches.	Coal. Pounds.	Feed. Pounds.	Water Meter Readings. Cubic Foot.	Barometer Inches.	Thermometer attached to the Barometer. F.
9 A. M.	54	·1	3·75	1,120	3,040	Meter out of order.	30·07	94·
10	48	·1	4·	. . .	1,900		30 07	95·5
11	40	·1	3·875	420	1,900		30·07	95·5
12 M.	41	·1	3·	140	1,520		30·07	97·
1 P. M.	58	·25	4·	420	1,900		30·10	96·
2	59	·3	2·5	. . .	1,140		30·06	96·
3	42	·3	2·06	280	1,900		30·07	96·3
4	49	·25	2·5	280	1,900		30·	96·5
5	62	·3	1·5	280	1,520		29·96	96·
6	26	·4	3·125	. . .	2,660		29·97	96·
7	60	·4	3·25	280	1,520		29·97	96·
8	55	·5	2·875	280	1,900		29.95	97·5
9	55	·5	3·5	280	2,280		30·	96·
10	50	·45	4·5	. . .	1,900		30·	96·5
11	61	·4	4·	. . .	1,520		29·99	94·5
12 Mid.	. .	. . .	. . .	. . .	1,140		. . .	. . .
	$\frac{760}{15}$	$\frac{4·45}{15}$	$\frac{48·435}{15}$	3,780	29,640		$\frac{450·35}{15}$	$\frac{1439·3}{15}$
	= 50·7	= ·3	= 3·23				= 30·02	= 96
	Coal weighed back, =			88			=29·839	
				3,692			at 32° F.	

Time.	Temperature Fahrenheit.					
	Feed.		Fire Room.		Flue.	
	Initial.	Final.	Dry Bulb.	Wet Bulb.	Thermometer.	Pyrometer. (Uncorr'ted Readings.)
9 A. M.	75	200	104·3	95·	322	400
10	76	210	107·2	100·	310	400
11	76	210	108	98·	282	380
12 M.	76	210	109	98·	276	370
1 P. M.	76	212	106·5	96·	293	385
2	77	195	105	96·5	. .	390
3	77	210	108	100·	. .	385
4	. .	. .	110	101·5	. .	375
5	76	190	109·5	99·	. .	380
6	76	210	108	98·	278	380
7	77	205	108	97·5	306	415
8	76	210	106·5	97·5	314	415
9	77	205	109	98·5	294	400
10	77	205	105	95·	290	390
11	78	125	104·5	95·	288	390
	$\frac{1070}{14}$	$\frac{2797}{14}$	$\frac{1608 \cdot 5}{15}$	$\frac{1465 \cdot 5}{15}$	$\frac{3253}{11}$	$\frac{5855}{15}$
	= 76·4	= 199·1	= 107·2	= 97·7	= 295·7 Probable mean all the time, 293°.	= 390·3

REMARKS.

Hauled fire at 7.37 A. M.

Steam pressure, 60. Water gauge, 4 inches.

Started fresh fire at 7.43 A. M., with 338 pounds of wood, equivalent to $\cdot 4 \times 338 = 135 \cdot 2$ pounds coal.

Steam pressure, 60. Water gauge, 3·5 inches.

Trial ended at 12 mid.

Steam pressure, 48. Water gauge, 4 inches.

Commenced to feed at 7.54 A. M.

Put on first coal at 7.56 A. M.

Fired 29 times during the test.

Sliced the fire 14 times.

Cleaned the fire 4 times.

Contents of the furnace and ash pit weighed at the end of the test,	994	pounds.
Picked out unburned coal,	417	"
Ashes,	577	"

Made two tests of furnace temperature, heating 1·2645 kilogrammes of wrought iron in the furnace, and putting it into 100 pounds water, raising the temperature—

In experiment 1,	11°·5 F.
In experiment 2,	12°· F.

	Pounds.
Coal fed,	3,692·
Wood equivalent,	135·2
	3,827·2
Picked from ashes,	417·
Coal burned,	3,410·2
Moisture in the coal, 3,827·2 × ·0355 =	136·
Dry coal burned,	3,274·2
Ashes,	577·
Combustible burned,	2,697·2

Through inadvertency, the level of the water in the boiler was ½ inch higher at the end of the test than it should have been. On the other hand, as steam was drawn from the boiler for use, the pressure became too low to end at 60 pounds as desired, and the trial was actually ended when the boiler ceased to make steam of the pressure of 48 pounds per square inch, *i. e.*, 12 pounds per square inch lower than at the start. As these two errors neutralize each other to a great extent, and as the general results of the trials differ enough to show at a glance that the conclusions to be drawn from the comparison of such results are independent of minor corrections, it was deemed unnecessary to take these into account.

The following is Mr. E. E. Magovern's record of his calorimeter experiments—

Number of Test.	Weight of Water in Calorimeter. Pounds.		Temperature of Water. F. Degrees.		Steam Pressure Pounds per Square Inch.	Duration of Test. Minutes.
	Initial.	Final.	Initial.	Final.		
7	308·5	339·75	77·3	180·3	52	3·067
8	306·5	327·	77·2	147·8	48	2·079
9	306·2	321·95	76·3	127·	43	1·567
10	306·2	320·95	76·	128·	45	1·579
11	309·8	342·3	77·2	186·	54	3·054
12	310·6	329·6	77·	136·3	62	1·496

Item.	Before Change.	With Post and Sawyer Attachment.
Duration of test, hours,	16·	16·28
Wood for starting fire, pounds,	323·5	338·
Coal equivalent of this wood, pounds,	129·4	135·2
Coal put into furnace, pounds,	4652·	3692·
Moisture in coal and wood, pounds,	184·1	136·
Unburned coal picked from ashes, pounds,	713·	417·
Ashes, pounds,	548·	577·
Per cent. of Ashes,	14·1	17·6
Coal burned, including wood equivalent, pounds,	4068·4	3410·2
Dry coal burned, including wood equivalent, pounds,	3884·3	3274·2
Combustible burned,	3336·3	2697·2
Times furnace was fired,	23·	29·
Times fire was sliced,	8·	14·
Times fire was cleaned,	1·	4·
Draught pressure, inches of water,	·14	·30
Water pumped into boiler, pounds,	37620·	29640·
Temperature of feed water, Initial,	62·7	76·4
Temperature of feed water, Final,	187·9	199·1
Steam gauge, pounds per square inch,	50·9	50·7
Barometer, inches of mercury at 32°,	30·05	29·839
Temperature of fire room, dry bulb,	110°·6	107°·2
Temperature of fire room, wet bulb,	95°·9	97°·7
Height of water in glass gauge,	2·84	3·23
Flue temperature,	340°·	293°·
British thermal units imparted by the boiler to the steam generated per pound (by Mr. E. E. Magovern's calorimeter experiments),	958·9	940·4
Real evaporation from actual feed temperature at actual pressure, (by Mr. E. E. Magovern's calorimeter experiments),	35474·	27721·
Equivalent evaporation from and at 212° Fah.,	37340·	28852·
Pounds per hour, Real evaporation, actual condition,	2217·	1703·
Pounds per hour, Equivalent evaporation, at and from 212,	2334·	1772·
Pounds per hour, Dry coal,	243·	201·
Pounds per hour, Combustible,	209·	166·
pounds per hour per sq. foot of grate surf. Real evaporations, actual conditions,	88·7	85·1
pounds per hour per sq. foot of grate surf. Equivalent evaporation,	93·4	88·6
pounds per hour per sq. foot of grate surf. Dry coal,	9·71	10·05
pounds per hour per sq. foot of grate surf. Combustible,	8·34	8·28
Pounds per hour per sq. foot of heating surf. Real evaporation, actual conditions,	1·967	1·363
Pounds per hour per sq. foot of heating surf. Equivalent evaporation,	2·070	1·418
Pounds per hour per sq. foot of heating surf. Dry coal,	·215	·161
Pounds per hour per sq. foot of heating surf. Combustible,	·185	·133
Boiler horse-power, rating a horse-power at an evaporation equivalent to 30 pounds of water from and at 212° F.,	77·79	59·07
Fuel per hour per boiler horse-power, pounds, Dry coal,	3·121	3·404
Fuel per hour per boiler horse-power, pounds, Combustible,	2·680	2·803
Square feet of heating surface per boiler horse-power,	14·49	21·16
Square feet of grate surface per boiler horse-power,	·321	·338
Real evaporation actual conditions, pounds, Per pound of dry coal,	9·133	8·466
Real evaporation actual conditions, pounds, Per lb. of combustible,	10·633	10·278
Equivalent evaporation, pounds, Per pound of dry coal,	9·613	8·812
Equivalent evaporation, pounds, Per pound of combustible,	11·192	10·697

COAL.

The coal used was Scranton coal, stove size, apparently quite dry.

From each barrow weighed during each test, we took a few pieces, and set them aside in the room adjoining the fire room. At the end of the test, we took about 3 cubic feet of the thoroughly mixed pieces of coal, boxed it up and sent it to the laboratory of the Stevens Institute, where we made an average sample, each test, by breaking with chisel and hammer a lot of fragments of 30 different pieces, the sample being then powdered and bottled in a well-stoppered bottle.

For the analysis of the coal, we adopted the scheme described in pp. 102–105 and 135–136 of the third edition of *Notes on Assaying and Assay Schemes*, by Pierre de Peyster Ricketts, according to which we carried through two analyses, agreeing together throughout to within $\frac{1}{1000}$ of the weight of coal analyzed, the record of the last analysis, made on May 10, 1887, being as follows:

	Grammes.
Coal + crucible + cover,	12·675
Crucible + cover,	10·675
Coal,	2·000

Placed the crucible and contents, cover on, in drying oven, heated to 105° C., keeping up that temperature for 15 minutes, cooled, weighed.

	Grammes.
Coal + crucible + cover before heating,	12·675
Crucible + contents + cover after heating,	12·598
Moisture,	·077

Heated 3½ minutes, cover on, over Bunsen burner, then 3½ minutes over blast lamp, cooled, weighed.

	Grammes.
Crucible + contents + cover before heating,	12·598
Crucible + contents + cover after heating,	12·486
Volatile and combustible matter + ½ sulphur,	·112

Heated over Bunsen burner, cover off, then over blast lamp, then ditto at the same time supplying oxygen gas, till the combustion was complete, constant weight, cooled, weighed.

	Grammes.
Crucible + contents + cover before combustion, ...	12·486
Crucible + contents + cover after combustion, ...	10·872
Fixed carbon + ½ sulphur, ...	1·614

Again,

	Grammes.
Crucible + contents + cover, ...	10·872
Crucible + cover, ...	10·675
Ash, including phosphorus, ...	·197

For the determination of the sulphur another portion of the powdered coal was used.

	Grammes.
Coal + crucible, ...	23·567
Crucible, ...	21·567
Coal, ...	2·000

which, after thorough fusion with the prescribed mixture of Na_2CO_3 and KNO_3 and treated in the usual way for the determination of S by estimating it as Ba SO_4, gave—

	Grammes.
Crucible + filter ash + Ba SO_4, ...	7·815
Crucible + ash, ... 7.677 .002	7·679
Ba SO_4, ...	·126

S = ·137 × ·136 = ·0186.

It so appears that the coal as fed to the furnace on May 5th and 6th contained in unit weight—

Moisture, ...	·0385
Volatile and combustible matter, ...	·05135
Fixed carbon, ...	·80235
Sulphur, ...	·0093
Ash, ...	·0985
	1·00000

By actual combustion in the furnaces, the so-called ash amounted to—

$$\frac{548}{4068 \cdot 4} = \cdot 13469$$

in the test before change, and

$$\frac{577}{3410 \cdot 2} = \cdot 16919$$

in the test with the Post and Sawyer attachment.

The moisture in the coal at the time of the test with the Post and Sawyer attachment was, by the same process above described, ·0355 per unit weight.

Hence, as disposed of by the furnace unit weight of the coal was respectively:

	Before Change.	*With Post and Sawyer Attachment.*
Moisture,	·03850	·03550
Volatile and combustible matter,	·05135	·05135
Fixed carbon,	·76616	·73466
Sulphur,	·00930	·00930
Ash + unburned carbon,	·13469	·16919
	1·00000	1·00000

CALORIFIC POWER.

We charged in a crucible 50 grammes Pb O, 1 gramme powdered coal, well mixed, covered with 20 grammes Pb O.

Heated this in a furnace to thorough fusion, in about ten minutes; cooled, broke the crucible, cleaned the button of lead reduced, and weighed it.

The theory of this assay is that—

$$2\,Pb\,O + C = C\,O_2 + 2\,Pb$$

$$2\,[206{\cdot}4 + 15{\cdot}96] + 11{\cdot}97 = (11{\cdot}97 + 31{\cdot}92) + 2 \times 206{\cdot}4,$$

whence pure carbon should reduce a weight of lead

$$= \left\{ \frac{2 \times 206{\cdot}4}{11{\cdot}97} = 34{\cdot}4 \right\}$$

times its own weight, so that the multiplier for C. G. S. units of heat is

$$\frac{8080}{34{\cdot}4} = 235$$

nearly, and

$$Pb\,O + 2\,H = H_2\,O + Pb$$

$$(206{\cdot}4 + 15{\cdot}96) + 2 \times 1 = (2 + 15{\cdot}96) + 206{\cdot}4,$$

whence pure hydrogen should reduce a weight of lead

$$\frac{206{\cdot}4}{2} = 103{\cdot}2$$

times its own weight, so that the multiplier for C. G. S. thermal units in the reduction by hydrogen is

$$\frac{34462}{103{\cdot}2} = 334 \text{ nearly.}$$

It is also to be observed that if the heat is too intense, or if the fused mixture is allowed to be in the fire too long, there is rapid evaporation of the lead, and whereas too much reduction of lead would be impossible, the weight of the lead weighed is apt to be considerably short of the weight of the lead reduced.

We made eight assays, of which three were expected to be short on account of too intense heat, or of having had to keep the crucible in too long, owing to the temperature in the furnace not being high enough to accomplish rapid fusion, and the three, indeed, weighed much less than the average of all; and five, obtained under more favorable circumstances, giving weights about that of the last and mean of the five, as follows:

Grammes.	*Grammes.*
30·369	29·155
29·263	29·708
29·734	

It is usual to adopt the highest weight and calculate the calorific power of the coal, as if the reduction of the lead had been operated by carbon alone. If we should so reckon it, the result would be, in C. G. S. thermal units per gramme of the coal—

$$30{\cdot}369 \times \frac{8080}{34{\cdot}4} = 7133.$$

From the previous analysis, we know we have in one gramme of the coal as fed to the furnace in the first test

·80235	gramme fixed carbon, which at the rate of 8080 should give, ...	6483
(·05135)	gramme of volatile and combustible matter, whose chemical composition it would not pay for our present purposes to find out exactly; but which, judging from more complete analyses made by professional chemists, of coals resembling this and containing about the same per cent. of volatile and combustible matter, it appears safe to assume to be approximately equivalent to—	
·02	Hydrogen, which at the rate of 34462, should give, .	689
And		
·03135	Carbon, which at 8080, should give, ...	253
Also		
·0093	gramme sulphur, which at the rate of 2220 should give,	21
	Total heat of combustion, ...	7446

C. G. S. thermal units per gramme of coal, 7,744 C. G. S. thermal units per gramme of dry coal, 8,628 C. G. S. thermal units per gramme of combustible, 15,530 British thermal units per pound of combustible.

The assumption above in regard to the ·05135 of volatile and combustible matter, gives to unit weight of the coal, the following lead reducing material—

·02 hydrogen.
·83370 carbon.

and if the assumption is true, the lead reduced should be—

	Grammes.
·02 × 103·2,	2·064
·8337 × 34·4,	28·679
Total,	30·743
Whereas, our highest weight was,	30·369
Difference,	·374

equivalent to 10·8 milligrammes of carbon or 3·6 milligrammes of hydrogen per gramme of coal, really a very small quantity in work of such kind.

Assuming that ·015 of the volatile and combustible matter is hydrogen, or that the lead reducing material is

·015 hydrogen,
·8387 carbon,

the lead reduced should be—

·015 × 103·2,	1·548
·8387 × 34·4,	28·851
Total,	30·399

which practically agrees with the highest weight of the assays, as the difference even computed wholly in carbon would run into the deci-milligrammes. Taking then the ·05135 of volatile and combustible matter as equivalent to ·015 hydrogen + ·03635 carbon, the calorific power of the coal as used is—

·80235 × 8080,	6483
·015 × 34462,	516
·03635 × 8080,	294
·0093 × 2220,	21
Total,	7314

(*To be continued.*)

REPORT OF THE COMMITTEE ON SCIENCE AND THE ARTS OF THE FRANKLIN INSTITUTE, ON ALEX. E. OUTERBRIDGE'S METHOD OF CARBONIZING FABRICS, AND OF OBTAINING CASTINGS THEREFROM IN METAL.

[No. 1381.] HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, June 1, 1887.

The Sub-Committee of the Committee on Science and the Arts, constituted by the FRANKLIN INSTITUTE of the State of Pennsylvania, to which was referred, for examination,

MR. A. E. OUTERBRIDGE'S

method of carbonizing fabrics and casting therefrom,

Reports that Mr. Outerbridge's invention consists essentially in the preparation of organic textile fabrics, such as cloth and lace, and the natural organic structures of ferns, grasses, and the leaves of plants, so that they can be exposed to very high heats without causing a disturbance of their form by destructive distillation.

The material which has been subjected to this process becomes capable of resisting the action of the heat from molten cast iron, and may be used as a part of the surface, or facing, of a mould, for the purpose of producing an intaglio impression of the material in cast iron, or other metal, and which casting can be employed, in the manner of a die, for the purpose of reproducing the design, or figure, upon leather, wood, or metal.

The material to be used as a pattern, or mould, is first dusted with finely-powdered carbon, and then packed in finely-ground carbon within a refractory case, made of cast iron, graphite, clay, or other suitable material. This case is fitted with a lid sufficiently close to prevent such free access of air as would allow of combustion, but which is loose enough to permit the escape of the vapors and gases which are produced by the after treatment.

The case and contents are then placed in an oven, and subjected to a temperature of about 300° F. for several hours, until the escape of volatile matters from the case has ceased.

The case is then removed from the oven, placed in a suitable furnace or forge, and the temperature gradually raised to and kept

at a glowing heat for about two hours, after which it is removed from the fire and cooled.

When the contents are taken out of the box, they are shaken in order to remove the carbon with which they were dusted, and in which they were packed. They are then tested by heating to a white heat in a blast-lamp flame, and, if combustion ceases upon their removal from the flame, the operation of carbonization has been properly conducted, and they are suitable for use as moulds or facings of moulds for metal castings.

The success of the method of preparation consists in first removing the fluid or liquid parts of the structure operated upon, and then so slowly distilling off those constituents which are produced by destructive distillation, that the carbonaceous parts, which are unaffected by the heat, and the integrity of the fibres or structure of the fabric, shall not be disturbed by the too tumultuous exit of the vapors and gases; the exceedingly high temperature at the close of the treatment ensuring the complete expulsion of all volatilizable matter.

The difference of result obtained by heating organic structures from ordinary to high temperature, rapidly or slowly, to expel volatilizable parts, is well shown in the difference in the structure of cokes produced from ordinary bituminous coals in gas retorts, and that from the same coal in coke ovens.

Gas coke, which is made by rapidly heating the coal, is in the form of a spongy, porous mass, weak in structure, insonorous, easily ignited and having the appearance of a mass from which bubbles of gaseous matters have escaped.

Oven coke, which is made by heating comparatively large bodies of coal gradually and continuously for a long period, is of a close-grained structure, capable of sustaining great pressure, or weight, and so compact and dense that it is sonorous. This coke is far more difficult to ignite than the gas coke, and somewhat resembles graphite in its characteristics.

The fact that nearly pure carbon, which is with difficulty ignited, and having the ring of steel, can be produced from wood, flax, hemp, cotton, paper, or silk, by heating them to redness out of access of air in a carbon vapor, or imbedded in powdered charcoal, has long been known.

The committee has no knowledge of any other method of

carbonization, whereby the form or integrity of the structure of fabrics, either natural or artificial, can be preserved intact.

That the product of the method of treatment of organic structure by Mr. Outerbridge is a mixture of pure carbon with a silicious ash, is evident from the results of analysis.

A piece of ordinary muslin untreated yielded of

Moisture,	3·05 per cent.
Ash,	1·53 "

A part of the same strip of material when carbonized by Mr. Outerbridge's method weighed 34·26 per cent. of the untreated stuff, and it contained ninety-five per cent. of carbon. From these results, it appears that 65·74 per cent. of the original material was given off as water, vapors and gases. This carbonaceous residue was consumed with difficulty by the action of strong oxidizing agents, such as chromic and sulphuric acids, and required the use of a very high temperature, with free access of air, to burn it in the determination of an ash, which amounted in weight to 4·43 per cent. of the treated material.

The treatment has then converted an easily ignitible material into one exceedingly difficult of ignition and the method employed has secured at the same time a preservation of the integrity of structure with but slight diminution of dimensions.

The change in the properties of the carbonaceous residue from fabrics, by which it resists the action of highly-heated metal, similarly with graphite or graphitic carbon, permits the use of woven or natural fabrics as moulds, for the cheap reproduction in metal of designs, which could not otherwise be made, except at enormous expense for engraving.

The method of using the prepared fabric as a mould is very simple. When it is desired to pour the metal over it, the ends of material are fastened down, either by the edges of the flask or by pinning, and the gates arranged so that the metal shall flow over and not fall upon it. When the side of the block of metal is to receive the impression, the fabric is secured at the lower edge, and it is floated against the side by the inflowing metal.

When the treated fabric is brought in contact with melted cast iron, there appears to be no more tendency to absorption than there is with graphite. Several members of the committee were present when the exhibit accompanying this report was made in white cast iron.

The committee is informed that the most delicate fabric, such as a lace veil of fine mesh, can be stretched across a mould, and (if the metal be poured so as to rise equally upon both sides of it) that it will remain intact, and when the metal becomes cold, a parting of the metal will be made upon the line of the dividing "veiling," each part having impressed upon it an intaglio image of the fabric.

Paper and card board treated by this method have been employed to divide the metal in a casting, where it has been desirable to do so, for instance, to break the continuity of the metal in the circumference of a circular casting, and thus diminish or prevent the strain otherwise produced upon the internal parts in casting.

The application in the arts of the invention of Mr. A. E. Outerbridge is at present limited to the cheap production of elaborate designs for the ornamentation of castings, or for the production of dies for embossing leather, paper or metallic surfaces, and also for the easy parting or dividing of metal in casting; but new fields of application will undoubtedly be opened when the method is brought into more general use.

The Committee considers that the process devised by Mr. Outerbridge is of sufficient importance in the arts to warrant an award for meritorious invention, and suggest that one of the medals in the gift of the INSTITUTE be awarded to him.

Respectfully submitted,

CHARLES M. CRESSON, M.D., *Chm.*
W. P. TATHAM, C. CHABOT,
H. R. HEYL, ISAAC NORRIS, JR.

Amended by the award of the John Scott Legacy Medal and Premium, and as so amended,

Adopted, October 5, 1887.

WM. D. MARKS, *Chairman.*

Certified as correct. WM. H. WAHL, *Secretary.*

BOOK NOTICES

QUANTITATIVE CHEMICAL ANALYSIS BY ELECTROLYSIS. By Dr. Alexander Classen. Translated from the sécond German edition, by William Hale Herrick. New York: John Wiley & Sons. 1887.

A student in chemistry once asked his Professor what books he would advise him to get that would contain the latest discoveries and improvements. He was told that it was not so much a question of buying books, as of subscribing to scientific journals. This, of course, is very true; but the difficulty is that the quantity of material now contained in these journals has grown to such an enormous extent, that to hunt through them and sift out the desired facts consumes time that is very valuable to the busy worker. This is particularly true with beginners, and it is certain to be a source of embarrassment to them in their progress in so difficult a science as is chemistry. It is important, therefore, that attention should be drawn to the publication of any work that overcomes this difficulty.

Prof. Classen's treatise has to do with a line of chemical analysis that has heretofore been but little taught in our colleges. Indeed, it is within only the last five or ten years that electrolysis has been applied to chemical analysis to any extent. Prof. Classen has had a great deal to do with its development, as he has been the leading specialist upon the subject. His book is an octavo of 170 odd pages, well illustrated and clearly printed. A very good description is given of the various forms of batteries, dynamos etc.; also, of the apparatus used in distributing, reducing, and measuring the current. A thorough description of the determination of the metals follows, and a more elaborate one of their *separation*. A number of test analyses conclude the volume. The translator gives a variety of practical points and suggestions, resulting from his own experience. Thanks to his familiarity with the subject, therefore, the American edition is in some respects superior to the German original.

The work is one that will be of value to every chemist, if for no other reason than that it is the only treatise on the subject in either the German or English language. H. P., Jr.

ELEMENTARY TEXT-BOOK OF PHYSICS. By Prof. W. A. Anthony, of Cornell University, and Prof. Cyrus F. Brackett, of the College of New Jersey. pp. 527. New York: John Wiley & Sons. 1887.

In this volume, Profs. Anthony and Brackett have completed the work, entitled *An Elementary Text-Book in Physics*, a portion of which was published in 1884.

The first issue, which was styled by the authors Part I, treated of Mechanics and Heat. The book is now completed by the addition of Magnetism and Electricity, Sound and Light.

The completed work differs in many respects from other standard textbooks on Physics. We miss in it the cuts of apparatus generally employed in

illustration of the various branches of Physics. Though space is thus gained, and the text-book kept within reasonable limits, we fear that the departure will somewhat limit the usefulness of the work, since a good cut of a standard piece of apparatus goes far, even with a comparatively limited explanation, to elucidate the principles it is intended to illustrate. Where diagrams have been employed by the authors, they are so aptly and clearly described that the absence of the usual cuts is made the more marked.

The order of treatment of topics, viz., Heat, Magnetism and Electricity Sound and Light, does not seem to be as natural as that in which sound and light, precede heat and electricity. It would seem more logical to first discuss the more palpable wave motions concerned in the production and propagation of sound, to follow such discussion by the more subtle waves in the luminiferous ether that cause light, to follow this by the study of the effects of heat, and finally complete the work with magnetism and electricity. This latter study, it would seem especially preferable to defer until a general acquaintance is had with the other departments of physics. However, the order followed by the authors is not devoid of advantages, and may be preferred by some teachers.

Apart from the order of arrangement, it is quite evident that the book is the work of practical teachers. Throughout its pages the phraseology is such as to conclusively show that it was born in the class room. The presentation of the facts is strictly logical, and indicates that long and intimate familiarity that comes alone with repeated teaching. The authors are especially to be congratulated on their success in combining that clearness and conciseness in their text that are so markedly absent in so many advanced text books.

We heartily commend the work for the use of high schools and colleges.

E. J. H.

Flow of Water in Open Channels, Pipes, Sewers, Conduits, etc. By P. J. Flynn, C.E.

This little compendium of hydraulic formula and tables constitutes No. 84 of the *Van Nostrand Science Series*. In it the author has condensed and compared the several formulæ used for the service indicated in the title, and tabulated the constants so as to save time and labor in calculating the discharge under given conditions of slope, diameter, roughness, wetted perimeter, etc.

For accuracy and despatch, we incline to the opinion that the graphical methods are to be preferred for reading off results where such methods are applicable, and since the publication of the diagram* prepared by R. Hering, C.E., giving the results of the Ganguillet-Kutter formulæ, there is very little room for labor-saving improvements. C. E.

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* See *Transactions Am. Soc. of Civil Engineers*. 1878.

SCIENTIFIC NOTES AND COMMENTS.

METALLURGY.

TABLES SHOWING RELATIVE VALUES OF IRON ORE CONTAINING DIFFERENT PERCENTAGES OF IRON AND SILICA.—BY JOHN M. HARTMAN, M.E.

Percentage of Silica.

Fuel, $2 per Ton.

Percentage of Iron. Per Cent.	5 Per Cent.	10 Per Cent.	15 Per Cent.	20 Per Cent.	25 Per Cent.
40	6·47	5·76	5·05	4·32	3·50
45	6·75	6·11	5·43	4·80	4·06
50	6·99	6·38	5·77	5·16	
55	7·22	6·67	6·12		
60	7·44	6·88			
65	7·66				
70	7·90				

Percentage of Silica.

Fuel, $3 per Ton.

Percentage of Iron. Per Cent.	5 Per Cent.	10 Per Cent.	15 Per Cent.	20 Per Cent.	25 Per Cent.
40	5·61	4·61	3·81	2·90	1·85
45	5·92	5·12	4·24	3·43	2·45
50	6·18	5·40	4·66	3·94	
55	6·42	5·70	5·04		
60	6·64	6·06			
65	6·88				
70	7·12				

Percentage of Silica.

Fuel, $4 per Ton.

Percentage of Iron. Per Cent.	5 Per Cent.	10 Per Cent.	15 Per Cent.	20 Per Cent.	25 Per Cent.
40	4·76	3·68	2·58	1·48	·20
45	5·08	4·02	3·06	2·23	1·05
50	5·35	4·46	3·56	2·72	
55	5·65	4·77	3·95		
60	5·85	5·16			
65	6·09				
70	6·34				

EXPLANATION OF TABLES.

Table showing price that can be paid for sufficient ore of different percentages of iron and silica to make one ton of iron.

Rule.—To the price shown in table corresponding to the given ore in iron, silica and price per ton of fuel, add the selling price of iron at the

furnace. From this deduct the constant number 12 which gives the cost of ore for one ton of iron.

Example :—Ore containing 40 per cent. iron, 25 per cent. silica, fuel \$3, iron \$16 at furnace.

\$1.85
16.00
\$17.85
12.00
\$5.85—cost of ore for one ton of iron.

Example :—Ore containing 65 per cent. iron, 5 per cent. silica, fuel \$4, Iron \$20 at furnace.

\$6.09
20.00
\$26.00
12.00
\$14.09—cost of ore for one ton of iron.

To find the cost of one ton of ore divide the cost of ore per ton of iron by one of the following factors :

FOR ORE CONTAINING IRON.

40 per cent.	45 per cent.	50 per cent.	55 per cent.	60 per cent.	65 per cent.	70 per cent.
2.50	2.23	2.00	1.82	1.67	1.54	1.43

These factors also represent the amount of ore in tons to make a ton of iron.

Example :—Ore 65 per cent., iron 5 per cent. silica, fuel \$4 per ton, iron \$20 at furnace gives \$14.09 for cost of ore.

1.54)14.09(\$9.15 cost per ton of ore.
13.86
2.30
1.54
7.60
7 70

This table applies to ores containing not over ·5 per cent. sulphur or manganese, to a plant thoroughly equipped with modern appliances making 500 tons per week with economical and efficient management.

PHYSICS.

THE PYRO-MAGNETIC GENERATOR.—The following item of correspondence, which we glean from the *Electrical Review*, September 24, 1887, has a direct bearing on the subject discussed in last month's issue of the JOURNAL, by Mr. Hering, viz.:

"I spent several years endeavoring to produce a practicable motor operating by the remarkable change of iron at a red heat, and brought the results of my investigations and my experimental apparatus before the Committee of Science and Arts of the FRANKLIN INSTITUTE in the winter of 1884–85, for an advisory report.

"The plan I proposed was the same as that adopted by Mr. Edison, namely, using laminated armatures, and alternately heating and cooling them

above and below the critical temperature at which the magnetic change occurs. I filed an application for a patent in the year 1884, but was much discouraged by the fact that at the high temperature oxidation of the heated iron would occur, notwithstanding a coat of plating or enamel.

"The idea of producing electricity upon this plan also presented itself to me; but as the same difficulty would have to be overcome, I considered it impracticable. I have, however, recently devised a plan for heating and cooling which prevents oxidation, and is applicable to a generator; and I am preparing an application for letters-patent in which I will incorporate a certain feature of my motor, which, I believe, was original with me.

"My investigations of the records brought to light an experiment of Prof. Gore, described in *Philosophical Magazine*, vol. xl, p. 173, 1870, in which a rod of iron having a coil at each end, one being connected with a battery and the other with a galvanometer, generated induced currents when the central portion of the rod between the coils was heated and cooled.

"It is true that he was examining the magnetic change in iron, and not endeavoring to produce a generator of electricity; but his apparatus was a generator as truly as Faraday's wire moving across the lines of force was a dynamo." WM. B. COOPER.

"*No 517 Locust Street, Philadelphia, Pa.*"

CHEMISTRY.

DYEING WITH CHLOROPHYL.—In the *Journal of the Society of Chemical Industry*, **6**, No. 6, are given the remarks of Dr. E. Schunk, F.R.S , on the above subject, as follows:

The author, after referring to a paper on the same subject, by M. J. A. Hartmann, and the report thereon by M. Cordillot, dit Luzy (*Bulletin de la Société Industrielle de Mulhouse*, xxvi, pp. 283–296), mentioned the difficulties to be encountered in endeavoring to fix the coloring matter—chlorophyl—on ordinary tissues, in consequence of its extremely fugitive character, and the slight affinity which it manifests for ordinary mordants. He then described a method by which these difficulties can to a great extent be overcome, so as to impart great relative stability to the substance without impairing its fine green color.

Having obtained solutions of compounds which contain, along with coloring matter, various metallic oxides, such as cupric and zinc oxides, he showed that these solutions impart very little color to cotton, silk, or wool, but that coagulated albumen, gelatin, and more especially the animal products yielding gelatin, such as skin, the ossein of bones, etc., readily attract the color, and are readily dyed when immersed in the solutions. Specimens of leather dyed in this manner were exhibited to the meeting, showing various shades of green, which, in day-light at least, are very pleasing, though by gas-light they share the fate of ordinary greens, and appear dull and dingy. These colors stand the action of soap and dilute acids in the cold, but are not materially improved by the treatment. In the opinion of the author, the tannin of the leather does not assist in fixing the color. The peculiar compounds of color-

ing matter with acids and metallic bases, which the author employed in his experiments, do not yield lakes by treatment with ammonia, being in themselves lakes, which dissolve in alkaline lyes without undergoing any change. It would be possible to modify the method so as to make it practicable on a larger scale, but the cost would probably prevent it being successfully applied, even if the colors obtained were sufficiently bright and intense to make success desirable. H. T.

ON THE TANNIN OF OAK WOOD.—By Dr. Carl Böttinger. *Liebig's Annalen*, **238**, 3.

Commercial oakwood extract is dissolved in water, the clear solution decanted and evaporated to dryness. This dry extract is powdered and treated with acetic anhydride, which forms an acetyl compound, which is separated from the coloring matter by solution in glacial acetic acid. The acetyl oak tannin is precipitated by pouring this glacial acetic acid solution into water. A repetition of the process is required, in order to get a pure product, and such a purification is attended with considerable loss. The analysis by treatment with magnesia gives the formula $C_{15} H_7 (C_2 H_3 O)_5 O_9$. This compound is insoluble in water, ether and alcohol, but is dissolved by acetic ether, chloroform and acetone. By heating with water in a sealed tube to 135° C., there results acetic acid and a red brown powder, which is insoluble in water, and is an anhydride of tannic acid. This powder is collected, washed with water, dried at a temperature not exceeding 25° C., dissolved in alcohol, evaporated, and the residue dissolved in water, and evaporated to dryness in a desiccator. The residue is oakwood tannic acid, a light brown powder, hygroscopic and easily soluble in water and alcohol. The ultimate analysis gave the compact formula $C_{15} H_{16} O_{11}$, but on heating to 135° C., two molecules of water were given off, so the formula becomes $C_{15} H_{12} O_9$, $2 H_2 O$. This tannin possesses some differences from that of the oak bark. H. T.

SKATOL FROM STRYCHNINE. C. Stoehr (*Berliner Berichte*, **20**, 1108). Skatol, which is a peculiar product of the decomposition of albumen in the intestinal canal or by fermentation, has been obtained by the anthor in the distillation of strychnine with lime. The reaction lends support to the theory that one of the two nitrogen atoms in strychnine appertains to a skatol or indol group, while the other is of a hydro-pyridin group. W. H. G.

GLYCERIC ALDEHYDE. E. Grimaux (*Bull. Soc. Chim.*, **47**, 885.)—When dry glycerin is mixed with the slightly energetic commercial platinum black, it soon acquires energetic reducing properties and an acid reaction. But the oxidation proceeds slowly, observations varying from twenty-four to 960 hours, showing that the reducing power (estimated as glucose) attains a maximum equivalent to thirty or thirty-five per cent. of the glycerin. A very active platinum black so energetically oxidizes dry glycerin, that incandescence immediately takes place, and a regular oxidation can only be produced when the glycerin is diluted with twice its weight of water; five grammes of glycerin may then be operated on, using ten grammes of platinum black. The reducing power of the product attains a maximum in between four and eight hours, after which it diminishes, while the acidity

increases. After six or eight hours' contact with the platinum black, the mixture is, therefore, exhausted with water, and the aqueous solution is evaporated to a small bulk in vacuo on a water bath. The reactions of the solution leave no doubt of the presence of an aldehyde; it reduces cupro-potassic solutions, and precipitates silver, in the form of a mirror, from ammoniacal silver nitrate. It becomes brown when boiled with caustic alkalies, or with lime or baryta water; it becomes heated when agitated with a solution of sodium acid sulphite, and alcohol precipitates from the liquid a gummy matter, from which, however, it has not been possible to obtain the aldehyde either with sodium carbonate or with sulphuric acid.

Glyceric aldehyde has the same composition as glucose, and its most important property is that of fermenting under the influence of yeast; carbon dioxide is disengaged, and alcohol is formed, the latter having been identified only by the iodoform reaction in the distilled liquid. The fermentation takes place slowly, and is never complete, this being probably due to the presence of an excess of glycerin, which retards greatly the fermentation of glucose.

Whether the product is really glyceric aldehyde, or a glucose of the formula $C^6 H^{12} O^6$, is at present impossible to decide, and the author has only published his results as a measure of priority, Fischer and Tafel having obtained hydrazinic derivatives of the same aldehyde. W. H. G.

THALLIUM IN PLATINUM. H. N. Warren (*Chem. News*, **55**, 241).—After having detected thallium in platinum wire and foil by spectral analysis, the author endeavored to separate the metals. Ten grammes of the wire was dissolved in nitrohydrochloric acid, and after evaporation to dryness and resolution in very dilute nitric acid, hydriodic acid was added, and the thallium precipitated as iodide. The proportions of thallium found varied from 0·02 to 0·1 per cent., the wire containing more than the foil. Alloys of platinum and thallium containing 0·5 per cent. of the latter are useless for wire, and two per cent. thallium gives an alloy fusible at a red heat. W. H. G.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, October 19, 1887.*]

HALL OF THE INSTITUTE, October 19, 1887.

MR. JOSEPH M. WILSON, President, in the Chair.

Present, 114 members and eighteen visitors.

Additions to membership since the previous meeting, eight.

The Secretary reported, on behalf of the Committee on Science and the Arts, the award of the ELLIOTT CRESSON Medal to THOMAS SHAW, of Philadelphia, for his System and Apparatus for Automatically Testing Mine Gases, and for Signalling in Mines.

Mr. JOHN CARBUTT, of Philadelphia, gave an oral account of the various plans that had been proposed for taking instantaneous photographs at night, or in tunnels, etc., by the use of an artificial "flash" light; and described and gave a demonstration of the latest plan for this purpose, in which a mixture of gun-cotton and pulverulent magnesium is employed as the source of the "flash" light. Mr. CARBUTT'S remarks have been referred for publication.

Prof. GEORGE FORBES, F.R.S., of London, England, by invitation, described a new apparatus of his invention, intended for the measurement of electric currents, which he termed a "coulombmeter," and gave a practical demonstration of its use. Referred for publication.

The Secretary reported that since the last meeting, the INSTITUTE had suffered the loss by death of JOSEPH E. MITCHELL, one of its Vice-Presidents; and of HECTOR ORR, one of the oldest members of the INSTITUTE, and for many years a member of the Board of Managers. He stated that the Board had taken measures to have prepared and published in the JOURNAL a suitable memorial of the deceased members.

Mr. SAMUEL R. MARSHALL was elected to the Board of Managers for the unexpired term of HECTOR ORR, deceased.

An election to fill certain vacancies in the Committee on Science and the Arts resulted as follows:

Mr. JOHN R. MCFETRIDGE was elected in place of HECTOR ORR, deceased.

Mr. CARL HERING was elected in place of Dr. N. A. RANDOLPH, deceased.

Mr. H. W. SPANGLER was elected in place of Mr. E. A. GIESELER, resigned.

PROF. LEWIS M. HAUPT made some remarks upon the incongruity at present existing in the assignment of the work of the numerous scientific and engineering bureaux to certain departments of the Government, and commented upon the consequences of this absence of proper system, and on the desirability of a change. He offered a series of resolutions, stating the foregoing facts, and urging upon the National Congress the necessity of a reform.

The subject was referred to a committee with directions to make a report at the next stated meeting.

Mr. HENRY G. MORRIS, of Philadelphia, exhibited and described a novel and ingenious form of traveller for drawing electric wires through conduits. Referred for publication.

The Secretary exhibited a remarkable specimen of blacksmiths' work, in the form of a beautifully-symmetrical goblet of steel, entirely hand-forged—without boring or drilling—from a one and one-half-inch round bar of steel. The maker is Mr. FRED'K SEARLE, in the employ of the Simond's Roller Machine Company, of Fitchburg, Mass.

Adjourned. WM. H. WAHL, *Secretary*.

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CRUCIBLES of BLAST FURNACES.

By John M. Hartman.

[*A Lecture delivered at the* Franklin Institute, *Friday, January 28, 1887.*]

The Lecturer was introduced by the Secretary, and spoke as follows:

In a furnace for producing pig iron, the lower part is divided as follows:

(1.) The bosh or slope down to the tuyeres.

(2.) The crucible from the tuyeres down to the hearth.

(3.) The hearth, or bottom.

The bosh consists of, usually, an eighteen-inch wall with a series of inch cooling pipe against the outside of the wall, and a wrought-iron bosh jacket against the pipe. This jacket supports the walls and prevents them from spreading, while the coil with its water circulation maintains the walls an uniform thickness of about twelve to

fourteen inches, the first four to six inches melting off until the chilling influence of the coil prevents any further melting. It is better to use a thirteen and one-half–inch wall at once, but the conservatism of furnacemen demands eighteen inches, thirteen and one-half inches has been tried and does well. From the bottom of this bosh jacket to a foot below the tuyere is placed a cast-iron tuyere jacket with one-inch cooling pipe cast in it. It has the requisite number of holes for the tuyeres and breasts, while under each tuyere is placed a small cinder hole to bleed the tuyeres in event of a pocket forming in front of the tuyere and retaining the cinder which, if not drawn off, will close the tuyere. The wall inside of this jacket is eighteen inches usually, but could be thirteen and one-half inches, same as on the bosh. This crucible jacket is made in sections held together with key bolts and has a trough at the bottom to hold water and chill the crucible wall below. Each tuyere opening has a projection extending inside of jacket, which holds and retains the tuyere breasts in position, independent of the walls if they should melt away. This obviates the old trouble of a tuyere drooping at the nose when the walls melt.

From the trough of the tuyere jacket, down to twenty-four inches below the hearth, is placed a heavy wrought-iron crucible jacket made in sections and clamped together. At the front is placed the dam or iron notch, at the back is placed the cinder notch, both being supported independent of the walls by the jacket. Near the back of this jacket, at the hearth level, are placed two small iron notches for purposes to be hereafter explained. The wall inside of this jacket consists of three nine-inch courses of best fire brick, and between each of them the vertical joints are made two inches thick, composed of crushed fire brick and clay driven tight in these vertical joints. These joints prevent any cracking of the brickwork of each nine-inch course from extending through to its neighbor, the object being to prevent the iron from leaking through to the jacket. A similar joint, but without fire clay, is placed between the outside course of brickwork and the crucible jacket.

The hearth or bottom is built in the foundation of the furnace, and is composed of the best fire brick interlocked to prevent the iron floating them up. The centre of the hearth inside of the crucible is depressed about sixteen inches, forming a pool for the

iron, which being filled up remains during the blast. The ground around the crucible jacket is usually filled up, say, twelve inches above the hearth.

The fuel passing down the bosh is oxidized at the tuyere, converted to carbonic acid, and is immediately reconverted by the glowing fuel to carbonic oxide. At a level of about three feet above the tuyeres, there practically exists a strong reducing atmosphere of carbonic oxide. This atmosphere of carbonic oxide prevents the intensely hot iron sponge or reduced ore further up the bosh from burning back to the oxide of iron, as the sponge at this high heat will quickly decompose carbonic acid and seize the atom of oxygen from carbonic acid. This atom of oxygen with the atom of iron will appear in a brownish red smoke at the chimney top. The lower part of the furnace is filled with glowing coal, through which the shots of molten cinder and iron fall, and are collected in the crucible. The liquid cinder being lighter, floats on the molten iron until it rises to the level of the cinder notch where it is drawn off. The cinder notch is placed back of the furnace to equalize the heat across the hearth and keep the iron in the back part of the hearth in a fluid state. When the iron and cinder notch are both front, the excess of heat melts away the walls, keeps the front tuyeres hotter, which causes more blast to enter, and makes the furnace drive faster on the front. When the iron has risen nearly to the level of the cinder notch, it is drawn out through the dam at the front. The iron tap hole in the dam is stopped with a large ball of clay, which is dug out when the iron is tapped off. Adjoining the iron notch is a large cast-iron trough washed with clay through which the iron flows to the skimmer at the end of the trough. This skimmer separates the cinder or any dirt floating on top of the iron, leaving the iron flow into the pig bed bright and clean.

Around the bosh, below the mantel, is placed the circular-blast distributing pipe, having a branch to each tuyere. The connection between the tuyeres and branches are made with a globe-faced flexible tuyere pipe. The tuyeres are spaced evenly and their number is determined, (1) by the volume of air; (2) by the diameter of the nose, which should not exceed four and one-half inches for anthracite, and five and one-half inches for coke; this allows for four-inch and five-inch nozzles, respectively. The

velocity of the blast (engine measurement) for anthracite should be 15,000 feet per minute, and for coke 10,000 feet. With a slower velocity of blast, the cinder clogs the nose of the tuyere quickly.

These rules give more tuyeres than in the old practice, and smaller nozzles, but it admits of a larger hearth, disseminates the blast more evenly across it, and lowers the zone of fusion by the increase in size of hearth. An excess of tuyere area allows greater part of the blast to pass those tuyeres that work most freely, and the hotter the fuel near a tuyere the greater is the consumption of air at that tuyere. The result is, that this particular tuyere will take, say, twice its volume of air and rob the others to that extent. Sometimes two or more will combine and rob the rest. When this occurs, the heat works up the bosh, tilts or pushes up one side of the zone of fusion, causes irregular working, and forces the heat up that side of the furnace to the top, the gas escaping without doing its duty in reduction, and robbing the furnace of its heat. These passages upward are constantly occurring, and are worse with large tuyeres. They can be minimized by smaller tuyeres. The existence of these passages can be proven by placing at equal distance, say, six pyrometers in the side of the furnace at stock line, when it will be found that the different pyrometers will vary 200° from time to time, by the heat shifting around just as one or the other set of tuyeres takes the blast, and also by the channels cut in the furnace lining, which show when the furnace goes out of blast.

These passages throw the zone of fusion out of position, let raw stock down through that zone, producing irregular iron scouring cinder, and burning these particular tuyeres by the great flow of iron over them. The tuyeres must be so arranged and proportioned that they burn off the descending fuel column in such manner that across the top of the bosh the column will flow down evenly. To do this the tuyere must burn off the descending column at the bottom of bosh more rapidly at the bosh walls than in the centre, owing to the contraction of the bosh. To burn the fuel more rapidly at the bosh walls requires them to be well protected from melting, hence the use of the water coil before mentioned between the bosh jacket and the bosh walls.

The diameter of the crucible is determined by the volume of blast and the fuel, the tuyeres must be so proportioned that while they burn the fuel more rapidly at the walls, yet the blast must have

sufficient projection to reach the centre of the fuel column and prevent a core of fuel. Nozzles must be varied to meet any change in the volume of blast to insure a proper penetration. Running a long bar through the tuyere will soon show any core in the centre of the furnace.

The size of the nozzle determines the pressure to a limited extent only. It is the internal condition of the furnace that gives the pressure. This fact is too often lost sight of in speaking of nozzles.

Building a fire on the hearth at home, it burns slowly at first, as the walls absorb the heat so rapidly that the temperature is kept too low for perfect combustion of the gas passing off from the fuel. When the walls are fully saturated with heat, the fire burns vigorously, as, there being no heat absorption by the walls, the gas is burned to its full equivalent of heat. To keep this fire perfect, nothing is required but to add wood and remove the ashes. If some water is poured on the hearth stone, steam is generated, heat is absorbed, combustion is imperfect, the gas escapes unconsumed and the vigorous fire becomes dull. Again, let the fire burn fiercely and close the chimney damper, the gas now escapes unconsumed and the fire becomes dull. In the crucible of a blast furnace, when water leaks from a tuyere, heat is absorbed and the temperature lowered. This prevents perfect combustion at the tuyere, which also lowers the heat. If the crucible continues to cool off, the iron chills and eventually the cinder also. This cooling can also come from too heavy a burden or lowering the heat in the hot blast, etc.

Slacking the blast will heat up the crucible locally for a short time, but eventually will cool it if the equilibrium of heat absorbed and heat generated is lost by slower combustion. The blast must be increased fast as possible. If, on the other hand, the furnace scaffolds or chokes with dust, sufficient air cannot be got through, as the dust resists the blast pressure, and the heat generated being less than the heat absorbed, the equilibrium is lost and the crucible chills. This compares with the chimney and hearth before quoted. When the heat in the crucible is lowered, the zone of fusion soon feels the loss, and not melting rapidly enough, the disintegrated stock collects above it and increases the pressure, as will be shown elsewhere. This loss of heat makes the zone of fusion pasty and

sticky, which prevents the passage of the fuel as well as the ore and stone. The harder the fuel, the more difficult is it to restore the heat to the zone of fusion, hence the difficulty of anthracite practice.

There are two forces at work in the crucible.

(1.) Heat generation by combustion of the fuel assisted by the hot blast;

(2.) Heat absorption by the tuyere water and all water connections, by radiation from walls, conduction through hearth and foundation, by blowing out at cinder notch and iron notch, by the gas going upward, by the cinder and by the iron. The loss by tuyere water and all water connection by radiation and conduction is a constant loss going on, whether the furnace is running or standing. It consumes the heat from about one-sixth the total fuel used.

The deep hearth and thick crucible walls act as a regenerator to store up large volumes of heat and prevent the crucible from suddenly chilling. So great is this heat storage that the ground outside of the foundation is from 250° to 300° temperature three feet below the surface.

The blast or air entering the furnace is heated up to say 1,200° to 1,400° by regenerative stoves.* The temperature of the crucible above the tuyere, say, three feet, is about 2,900° when making No. 3 iron. Immediately in front of the tuyere there is a lower temperature caused by the entering blast, but at a short distance from the end of the tuyere, where the blue point of the blow-pipe flame is formed, the highest heat of carbonic acid plus the heat of blast, say, 6,000°, is reached. This heat is immediately lowered by the conversion of the carbonic acid to carbonic oxide. Above the tuyeres, for a distance of some feet, varying with the volume of blast, there is the bed of glowing coal extending from the zone of fusion down to the hearth through which the shots of falling iron and cinders from the zone of fusion percolate and fill

* A temperature of blast of 800° will ignite charcoal, 900° will ignite coke, and 1,300° will ignite anthracite. The porous character of charcoal and coke gives an interior combustion, while anthracite burns on the surface only, and one-eighth of an inch below the surface it is found untouched by combustion. Hence larger hearths and more surface of contact is required to consume it.

the interstices between the fuel in the crucible. In this percolation through the glowing fuel the unreduced oxide is reduced, but any escaping through the fuel is taken up by the slag unless it happens that the ore is very refractory and of greater specific gravity than the slag when it passes through the slag and is reduced by the carbon in the hearth. This destroys the graphitic carbon in the iron and lowers its quality.

These stray lumps of ore are occasionally found at the tuyeres, especially if the stock in the furnace is slipping. On the more perfect reduction of the oxide above the tuyeres depends the quality or number of the iron. The higher the heat the better the reduction.

That a certain amount of the oxide escapes reduction is shown by the slag analysis, and is due to the blast not working up uniformly at all points of the crucible. Cinder from No. 1 slag contains from traces to one per cent. protoxide, and from No. 5 iron two to eight per cent. protoxide. Variations from the above exist, owing to the presence or absence of sulphur, phosphorus, manganese, etc. The high heat caused by the larger amount of fuel used in making No. 1, and the contact of the iron with the solid fuel in the hearth reduces all oxide and changes part of the combined carbon in the pig to graphitic carbon, which is the cause of the better grade of iron. Care must be taken not to get too hot, or the carbon in its greed for oxygen will decompose the silica in the fuel to silicon, which, uniting with the iron, gives a soft weak pig, of a whitish fracture. The utmost skill of the founder is required here.

The glowing fuel in the crucible is gradually consumed in the formation of graphite or kish, by reduction of oxide of iron or by oxidation when blowing out after the iron and cinder are drawn off, and is replenished by the downward flow of the fuel.

The iron slag and fuel in crucibles is chilled at times by water leaking into them from the tuyere breasts or other water connections, by cold blast, by too heavy a burden of ore, and by long stoppages, etc. As soon as the furnace man finds the least indication of cooling, the burden of ore and stone must be lightened or the blast heat raised to restore the heat. Crucibles generally chill up to the cinder notch, but sometimes higher, when pockets are formed in front of the tuyeres by the blast. If these pockets do

not communicate with each other to let the slags flow around to the cinder notch, the holes in the jacket must be opened to free the tuyere from slag. Heretofore, this mass has been melted out by the flow of iron and cinder over it, requiring from one to four weeks to get rid of it and producing white and mottled iron during that time. At present, a coal oil blowpipe is used, which, in thirty hours, will restore the crucible to a normal condition. This blowpipe consists of a three and one-half inch clay tuyere, to which is attached a four-inch gas pipe seven feet long, and a section of four-inch hose to supply the blast. A one-fourth–inch pipe leads the oil from the tunnel-head into the blowpipe where it is vaporized by the rebounding heat in front of tuyere. The vapor passing into the blast current is ignited and directed against the iron notch, which it soon melts out, a little soda or salt being used to flux and assist the melting.

Before starting the blowpipe, the iron trough should be removed and a good bed of sand put in its place, as the melted material on cooling is extremely hard and fuses with anything with which it comes in contact. Only light kerosene or torch-oil should be used and must be well mixed with the air before burning. A pressure of four or five pounds of blast is required to project the flame well forward, and prevent burning the nose of the blowpipe. It is best to lay some old fire brick in front of the iron notch, or hole operated on, leaving through the brick an opening for the pipe to play through and slag to run out. These bricks reflect or throw back the heat into the hole, and cause a more rapid melting.

When the crucible is chilled above the tuyeres, one blowpipe is started on the cinder notch and one on one of the tuyeres nearest the cinder notch. In a short time, a passage is melted from the notch to the tuyere. The blowpipes are then put to work on the next tuyeres each side, and blast is turned on the tuyere melted out. As each tuyere melts out and communicates with the cinder notch, blast is turned on them, which, in a short time, melts out the crucible in front of the tuyeres. The chilled material is mostly fuel and cinder with a little iron, but should there not be fuel enough to keep up combustion, a spray of oil must be used, as hereafter explained.

It takes some time to get the opening hot and get the right proportion of oil, as there is generally too much used, which lowers

the temperature. As the opening becomes hotter, more oil can be used. A loud humming noise denotes when the proper quantity is used. A screen must be placed over the upper end of the oil pipe to prevent dirt choking the pipe and cock. A round way cock only should be used.

The melted part is removed as soon as it flows out on the bed of sand. If the fused material is allowed to chill before removal, a little dynamite judiciously applied will shatter it. Dynamite is also used to open a passage from a tuyere through the chilled material into the live coal beyond. It should only be handled by an expert. The two extra iron notches at the back of the furnace are then opened, and blowpipes started to melt them out. This melting proceeds on in until the centre is reached. By care, the mass can be melted out, leaving a roof above to support the cinder and iron flowing through the cinder notch, the blast being kept on the furnace during this time. The back iron notches are now closed, and the front blowpipe is directed up against the roof, melting it, letting the iron and cinder down, which flows through the iron notch, the cinder notch having been previously closed to accumulate a large flush of cinder. This large flush soon melts away the roof, and when the flush is out the iron notch is closed until another flush collects, when it is again drawn off by the iron notch, and repeated until the iron becomes gray, when the cinder notch can be opened as usual. The white iron made during this time flows sluggishly, and it is best to run it out with the cinder on top of the iron, which keeps it hot and gets it into the pig bed in better shape.

During the time the blowpipes are melting out the crucible, they are heating it up, so that on melting through the roof the cinder and iron are kept fluid in it. As the iron becomes grayer, the volume of blast is increased until No. 2 iron is reached, then the burden is increased until the normal burden is reached, when the full volume of blast is used. The heat must be built up step by step, as surely and as fast as possible.

The old iron left in the hearth by the chill must be carefully worked out before good gray iron can be made, as this old material being white iron, will absorb the graphite of the gray iron and lower its grade even when a light burden is used. This difficulty often causes the founder to lighten his burden again, when by a

little patience the iron would improve in quality as the old iron is worked out. A light burden is an evil that must be heroically met on the score of economy.

Without a clean hot crucible no good iron can be made, and special attention must be given at all times to maintaining the equilibrium of heat and proper temperature in it. At a certain heat, kish or graphite is formed in the crucible from the fuel, which gives a strong, dark iron. At a higher heat, graphite is replaced by silicon. As some of the graphite always escapes with the slag, it can be seen, and gives the furnace man notice of a good temperature for No. 1 and No. 2 iron, unless sulphur is in excess. When strict attention is paid to the heat, there is no difficulty in working titanic ores, as all they require is a crucible hot enough to melt the cinder, and not too hot, or the titanic acid is reduced, which clogs up the hearth. The two extremes must be avoided, and, with care, there is ample range between them. These titanic ores make excellent iron for forge purposes.

Crucibles are often chilled by a shower of granular and dusty stock, which, when at a bright red heat, flows like fine shot. These granules surrounding the fuel in the crucible prevent free access of air to the fuel, and combustion goes on slowly. The constant loss of heat from the hearths by tuyere water, radiation, etc., overbalances the heat generated, and the crucible chills. At a level, say, three to six feet above the top of the bosh, is the zone of fusion resting on the bed of glowing fuel below. This zone is constantly forming and melting away, and, taken as a whole, is in a pasty condition, the top being slightly pasty and the bottom liquid. When ores are used that disintegrate rapidly to a granular state, they rattle down through the stock ahead of the fuel they are charged with, and rest on the zone of fusion. If this zone does not melt the fine stock as rapidly as it accumulates, it piles up and, pressing heavily on the zone, retards the passage of the ascending gas. The blast pressure goes up with this accumulation of fine material, and when the engine is not powerful enough the furnace chokes and irregular working begins.

Blowing at as high a pressure as possible for some time will generally force an opening up through the zone, which relieves the pressure and causes the blowing engine to run off. The engine is slacked which reduces the pressure in the opening, and lets the

fine material down to the crucible as stated: at the same time part of the dust is blown over into the flues, and immense volumes escape at the chimney. While blowing against the zone of fusion the fuel below it consumes, if there is any escape of gas through

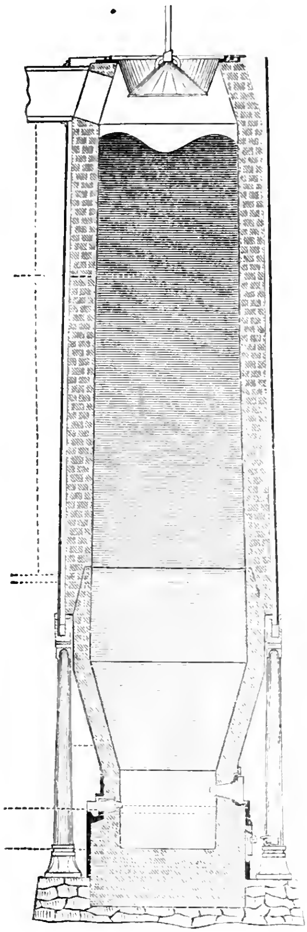

FIG. 1.

the zone, leaving a cavity, and the stock will not settle, the blast is thrown off suddenly, and if this does not cause the stock to settle, a hole is cut through the side of the furnace into the cavity and a torpedo exploded in it, which usually answers the purpose. This is a final resort, but the blowing must not be continued too

long, or there will not be coal enough left to restore the heat to the hearth. When the furnace slips or settles some distance, say ten to fifteen feet, the zone of fusion is disarranged, the cinder is forced back in the tuyere pipe, and the bosh chilled by the cold stock filling it. During this trouble, little or no gas passes the zone of fusion and escapes at the top of furnace to the stoves and boilers to make steam and heat the blast, and this just at the time it is the most wanted. Extra firing, at boilers and stoves, with its trouble and cost must then be resorted to. When the heat of the bosh, crucible, and stoves, are not too much exhausted, the heat can be restored to the zone of fusion

Fig. 2.

and the process becomes regular, but it often occurs that the edge of the zone of fusion around the walls chills, forming a ring which sets so firmly on the wall that it remains during the blast, and on going out the ring is found intact, while the brick above and below it have melted back from fifteen to twenty-four inches.

The opening spoken of, forced through the zone of fusion, is generally in the centre (see *Fig. 2*), but sometimes is found on the side against the wall. With this limited opening no good work can be done, and the obstruction must be removed by blowing it down with explosives, melting it out with the blowpipe through openings on the sides of the furnace, or by digging the stock out of the furnace. This trouble from fine material some-

times comes from a scaffold or scab on the bosh, which, by attrition or heat, becomes loose and leaves the fine stock back of it flow down to the crucible. This is not, however, preceded by a high blast pressure and a sudden easing up of the pressure. As soon as this dust material arrives at the crucible, it must be either melted and run out, or shovelled out through the tuyere breast. A small fine spray of coal oil through each tuyere that is working will help to melt the dust more rapidly and get the hearth hot quicker. This dust obstruction can also come from using too much fine ore; and slow driving with a large bosh favors its accumulation. In the absence of scaffolds we must look to the loss of heat in the zone of fusion for the cause of high pressure. With high blast pressures, the loss in leakage of blast is enormous, and the more leakage the less is the heat generated, while the constant loss goes on exhausting the heat of the crucible.

Running through part of the veins of anthracite and bituminous coal are streaks of bone and slate which, reaching the furnace, cause cold crucibles. There is less carbon in a ton of this fuel, while the bone is a highly refractory slag material, requiring a hot crucible to melt it. This bone, or slate, being of about the same specific gravity as the slag, follows down into the crucible after each cast, gradually filling it, except a pocket in front of the iron notch. As soon as this pocket fills with iron, the latter flows out through the cinder notch, melting the notch and spoiling the iron. Casting has to be resorted to probably every hour in this case. The blowpipes should at once be put to work on the extra iron notches back of furnace to melt the bone out. In many cases it lies in the crucible in a dry state and can be shovelled out. When a tuyere uses too much blast, it will sometimes build a lump of iron on the wall above it. The oxygen of the blast decarbonizes it, forming a pasty wrought-iron mass. At times this comes loose, slides down into the crucible, chills it more or less, and changes the quality of the iron.

When the zone of fusion is pushed up on one side, as before described (see *Fig. 3*), a pocket is formed above and back of it, which catches a large volume of dust and disintegrated material. In the course of time, when the increasing heat happens to work up to the low point of the zone, and melting that part off, a heavy shower of dust is left down to the crucible. It is usual to increase

the burden when the furnace begins to work *hot*, but in this case it must be lowered to give to the crucible the heat required to work off the dust. These dust showers did not occur so frequently in the old furnace practice, as they used a much smaller crucible and volume of blast, which allowed the stock to build up on the bosh and lessen its reducing and disintegrating capacity. The ores were broken uniformly, and all fine ores screened out. There are thousands of tons of this fine ore at old furnaces, covered with vegetation, which will be used in the future with stronger blowing engines.

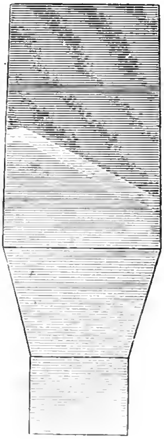

FIG. 3.

Fine ores reduce quickly and require rapid driving to prevent accumulation in the zone of fusion. Seven hours' exposure has been found sufficient to give No. 1 iron.

Higher up on the bosh large scaffolds form at times that come loose, slide down the crucible, exclude the fuel and chill the hearth. Damp weather will cool off a crucible, and if a heavy burden is carried, the barometer must be consulted and extra fuel charged in advance.

Using 10,000 feet of air per minute, a ton of coal per twenty-four hours is required to keep up the equilibrium of heat during a wet spell.

Since carrying heavier burden of ore and stone to a given weight of fuel has come into use, these dust troubles have increased and given rise to the expression, "too much burden"—which

implies loss of heat in the crucible to maintain the equilibrium in the zone of fusion. To an experienced founder, these dust troubles show beforehand, (1) by the gas; (2) by the pressure; (3) by the cinder; (4) by the iron; (5) by the charges driven in a given time and, knowing these with certainty, he can either warm up the crucible by higher heat of blast, or at once give the furnace a blank of fuel. Quick, prompt action in advance is required. With a hot crucible, the zone of fusion is shortened and the pressure is less. Heretofore, no attention has been paid to proportioning a furnace to use fine ore or an ore that disintegrates readily. Coarse, dense ores and fine ores should not be used together.

When a furnace begins to work colder, the back gas from the tuyeres shows white for a few seconds, when blast is thrown off, then black, and emits finely divided carbon in large volumes. As the furnace gets colder the quantity increases. This is accompanied by a higher pressure, and is due to disintegration of carbon.

Just here it will be well to call attention to the four filters or screens in the blast furnace.

(1.) The filter of cinder in the crucible, which allows the shots of falling iron to pass through it, and covering it carefully protects the iron from the oxidation of the blast;

(2.) The filter of fuel from the tuyeres to the zone of fusion, which takes up all oxygen of the air and carbonic acid, thereby preventing oxidation of the falling iron and any iron sponge passing the zone of fusion;

(3.) The zone of fusion acting as a filter allowing the fuel to pass through it as a solid, but liquefying the ore and stone. The pasty condition of the zone of fusion holds back the disintegrated stock, giving it time for reduction and fusion. If this pasty condition did not exist the fine ore would quickly rattle down through the stock to the crucible and floating on the cinder blanket, choke the crucible more or less. As part of the disintegrated ore would be reduced, it would quickly burn to oxide of iron and give red smoke at the chimney top, while the unreduced ore would melt, form finery cinder and pass out with the slag, causing a total loss of the fine ore; if any reduced ore passes through the slag, it changes the quality of the iron, as there is no carbon in it.

(4.) The dust and disintegrated material above the zone of fusion act as a screen or filter to intercept, to a great extent, the heat of

the ascending gas, and confine it to the zone of fusion below. That a certain amount of this fine material is required above the zone of fusion, is proven by the fact that on blowing in a furnace good work is not done until this accumulation takes place sufficiently to give a good working pressure. That also on shovelling out a healthy furnace that has been stopped by a break down, this material is found in great abundance about the top of the bosh.

The importance of the zone of fusion to the blast furnace and its laws are just being recognized. It must be maintained uniform if uniform iron is required, and must be kept as level as possible. It is extremely susceptible to any changes of heat below in the crucible.

This zone forms the dividing line between the reduction of the ores by carbonic oxides and the reduction by solid carbon. It might be called the digestive zone of the furnace.

Since rapid driving of furnaces has come into general use, it is found difficult to prevent the iron from melting through the crucibles, as they are much hotter from the larger volume of slag and iron falling into them. Cast-iron jackets, with water coil and thin walls, have been used, but the iron working through the cracks in the brickwork melted through the cast iron, and cutting down to the water coil caused explosions that burned the furnace men and destroyed the jacket.

The hearth also cuts down two to three feet, leaving the iron at a lower level. The iron trough is then lowered to get the iron. This is an error; it is better to let the iron lie in the bottom and let fresh iron accumulate on top of it to be drawn off at regular casting time, than to lower the hearth from time to time. At the end of the blast, the trough can be lowered, the iron drawn off, and a salamander in the hearth avoided. The iron tap hole, passing sixty to seventy tons of iron in fifteen minutes also gives way, burning the dam and sometimes cutting the coil, causing explosions that throw the molten metal in every direction. Watching a skilfully handled cast of sixty to seventy tons of iron, covering a space 30 x 100 feet, is a grand sight, but when this volume melts through the jacket, floods the building, explodes, scattering death among the furnace men, it is a different matter. Acts of heroism, surpassing those of the battlefield, have been performed by furnace men in trying to save their companions from such a terrible death.

In one case, forty-four tons worked out under ground into the cinder pit, when it commenced to boil, and drove the men out of the house. It was two hours before the men could approach the building, and it took forty-four hours to remove it and start the furnaces. The trouble to break up and remove the mass must be experienced to know its difficulty.

These boils are one of the curiosities of iron-making. A wet pig bed or a wet lump of clay as large as an acorn will start them, and if the iron is very hot they sputter and throw it all over the building. The peculiarity is that long after the moisture starting them is evaporated, the iron keeps up its molecular motion until it finally chills. Throwing water upwards in a fine spray and letting it fall on the metal chills the latter without an explosion.

The more recent improvements in connection with crucibles are as follows:

Heavy wrought-iron crucible jackets in sections extending from the trough of tuyere jacket down twenty-four inches below the hearth level, which is kept twelve inches above the ground Just below the hearth level and against the wrought-iron jacket is placed a six-inch pipe, making one coil around the jacket. One end of the coil is attached to the chimney of boilers and stoves, the other end terminates outside of the cast house, where it is attached to a small furnace. When the hearth is hot, cold air is drawn through this coil to chill the brickwork and prevent the iron melting through the jacket where it is buried underground and not exposed to the cooling influence of the air. When the crucible and hearth become cold, a fire is started in the furnace to warm them up and prevent loss from radiation and conduction. A hot crucible is the life of the furnace, and everything possible must be done to warm it up when it begins to lose its heat.

By taking it in time, serious trouble may be avoided. Laying the vertical courses with a space between them of pounded brick and clay, in connection with thicker walls and heavy jackets of wrought iron, prevents the melting through above the ground. This avoids all water connection below the tuyeres. The iron tapping hole six inches wide has on each side of it a false piece with water grooves in their backs, through which water trickles slowly to make moisture and chill the iron tap hole. An hour before cast-

ing, the water is turned off, and the moisture evaporates before casting. These false pieces are detachable, and readily replaced.

To avoid melting away the tap hole, it is stopped with clay, which melting, allows the iron to rush out in a large volume. Plumbago, in a cheap form, is now substituted, which resists the cutting action of the flow of iron. To bot up an iron tapping hole requires skill, and must be done quickly to get blast on the furnace with as little loss of time as possible. It must be well stopped back, and the plumbago worked out to the front edge of the false pieces, leaving an opening of say two and one-half inches in diameter, about half the depth of the stopping. This holds the iron in check, and saves the false pieces. The latest improvement is an air-cooled iron notch stopped with a hollow plumbago stopper. The stopper is filled with sand, which can be picked out quickly and a bar driven through the end of the stopper lets out the metal. The iron running through the hollow stopper wears it away, making place for another. This avoids the old stoppage and botting up. In connection with it is an automatic skimmer, which requires no watching or handling by the furnace men. The tuyeres are now inserted in the tuyere breasts with a metallic joint, and the tuyere breasts are secured to the tuyere jacket which holds them up to their work, even when all the brickwork inside the tuyere jacket is melted away.

Tuyere nozzles have been improved by making them of bronze and fitting them, metal and metal, to the tuyere nose, which prevents them from burning and allows them to be placed nearer the nose of the tuyere, thereby avoiding expansion of blast in the end of the tuyere. Nozzles with spiral grooves giving a twisting motion to the blast, are used when a strong penetration is required. These cause the blast to work further in by pushing the pieces of fuel apart. To detect quickly when blast is not entering all the tuyeres alike, a differential gauge is used, one to each tuyere pipe. To the circular blast pipe an inch pipe is attached, leading to the side wall of the cast house, where the gauges are placed and on one side of each is attached this inch pipe. In the connection of the tuyere pipe to the circular pipe is placed a reducer or bushing, sufficient to give a half pound back pressure or friction in the passing blast, and which serves to distribute the

blast at each tuyere more evenly. From each tuyere pipe connection, just below the reducer, is run a three-eighths–inch pipe and attached to the other side of the gauges. Mercury is placed in the tubes to half the height of the glass, the blast is then turned on. If blast is entering a tuyere rapidly, one column of mercury rises, the other falls, and if no blast enters the tuyere, both columns remain at the same height, as, the blast in the tuyere pipe being stationary, shows the same pressure in the blast as in the circular pipe. A simple inspection at any moment shows what each tuyere is doing, and whether the blast is working up one side of the furnace or evenly at all the tuyeres. When a gauge shows a tuyere is not taking blast, the furnace man must open it at once with the pricker rod and see that it gets its share of blast. The tuyeres are the life of the furnace and they must be kept free and open In the old practice, the bright tuyere was considered the best, but these gauges have proved the reverse, as the bright tuyere using but little blast makes an intense local combustion and glowing fire at the nose of the tuyere, but there is no penetration. Running a pricker rod through this glowing surface, the hard, cold fuel is found back of it.

Cinder notches (now universally used) are made in three parts, arranged to take apart readily, and when the furnace makes cinder rapidly enough, a continuous flow is used which, running at a moderate speed, does not destroy the notch, trough, or cinder cars. Opinions vary on this, but in time the continuous flow will be universal, even if a second cinder notch with larger opening is required to blow a flush from. The old practice of running by pressure of blast is now done away with, and running by revolutions of engine practiced. The volume of blast blown into the crucible, and not the pressure determines the volume of iron to be made. Care must be used with a crucible that has lost part of its heat as sufficient blast only to maintain combustion must be used, and as the fire gets hotter the volume can be increased. If too much is blown, the fire is blown out and the crucible chills. Patience and firmness will master the trouble.

The products of the crucible are:

The gases, the slag, and the iron. The gases are carbonic acid, carbonic oxide, nitrogen, the cyanides, and hydrogen, the two

latter being in small quantities. The constituents of the slag vary between the following limits:

Silica,	20 to 72 per cent.
Lime,	0 to 60 "
Magnesia,	0 to 34 "
Alumina,	0 to 30 "
Protoxide of manganese,	0 to 34 "

A slag making No. 1 iron from good stock runs silica 40, lime 25, magnesia 18, alumina and iron 17. In color, they vary from the creamy white or gray, No. 1 iron, to the glossy black from No. 5, with all intermediate shades. Dark green shows protoxide of iron present, in small quantities; light green shows protoxide of manganese; black shows large quantities of protoxide of iron and a heavy slag with a dull metallic lustre; reddish-gray black shows unreduced ores are melting and the furnace working raw. This alarms a furnace man more than any other trouble. When a scaffold is working off from the bosh wall a scaly reddish brown puffy cinder is formed. As a rule, a limpid cinder with white to gray fracture gives No. 1 or 2 iron if there is but little sulphur or manganese present. In working some ores, there is a curious exception; the last flush before casting must be a rough, dark slag to give No. 1 iron. With a limpid slag, the shots of iron pass quickly through it and are buried beyond the reach of the oxidation or decarbonization of the blast. A furnace at times will make two different slags, as one part of the crucible may be working off a piece of scaffold, or working cold, while the other part is working hot.

Where slag runs out and cools in runners, the out end on cooling pushes up in convex surfaces called bull's eyes, which, if they cool glassy on surface indicate want of lime.

The same test can be made by taking samples in cups, cooling them and breaking them when the fracture will show by any glassiness the want of lime. The quality of the iron being made at the time must be taken into consideration with the glassiness of the slag. Each furnace man must, by comparative tests, establish his guides, which old founders did well with before the days of analyses.

The tests used to determine whether there is lime enough in the slag, are (1), catching a small amount on a rod and drawing it out

in threads, which, if they snap off brittle, indicate lime enough; but if they are elastic, indicate that more lime is wanted; (2), by coating a rod, say one-sixteenth of an inch thick, letting it cool and examining its brittleness. Heat bleaches slag, and founders are often deceived in thinking a light-colored cinder is too limy. Crushing a cinder to powder and examining its color will show if unreduced oxide is present by its dark color. In building up the heat in the crucible, the cinder shows by its fluidity and color how it is progressing, and the first sign of a gray coating is watched for anxiously.

The slag shows alternate layers of black and white during this time. When too much lime is used, it can be quickly corrected by some quartz sand charged with the stock which soon works down to the zone of fusion. Samples of cinder taken from each flush and placed side by side during a shift, give a continuous history of the shift.

A slag that will run fluid and on cooling go to powder is of the following composition: Silica, 36·2; alumina, 11·0; lime, 48; magnesia, 1·4; calcium sulphide, 2·5; oxide manganese, ·5.

When sulphur is present in fuel or ore, the cinder must be basic to take up the sulphur. With rich ores containing sulphur, the cinder must be extra basic, as its small volume soon becomes acid in washing the sulphur out of the iron. This rule must be observed also with pure rich ores and sulphurous coke.

The flow of the slag, its limpidity and color are carefully watched by a good furnace man, as it gives with sound judgment a quick index to the interior workings of the furnace. Acid slags take less fuel, but give poorer iron and more of it. Basic slags take more fuel, give better iron, but less of it to a unit of fuel.

The third product of the crucible, iron, varies from No. 1, a black large open grain with sharp-pointed facets scratching the fingers, to No. 5, or iron with no facets. Nos. 1, 2, 3 are gray; No. 4 mottled or white, with gray spots, and No. 5 white. No. 1 iron is from a hot furnace, but if it works too hot the iron before spoken of is made, containing five to seven per cent. of silicon, soft, white, weak fluid (runs like water), and fit only for artistic work or light castings, requiring no strength. Carbon is what gives cast iron its strength and value, the other elements change its character for strength and weakness to a limited extent.

For a general rule, the sum total of the graphitic carbon and combined carbon is the same in all grades, but there is little graphitic carbon in No. 5, being nearly or all combined carbon, which is the cause of its extreme hardness and density. No 1 is high in graphitic carbon with little combined carbon, which is the cause of its softness and black color. With intense heat in a cupola and plenty of fuel white iron can be made kishy or gray.

One of the best brands of No. 1 foundry iron in the market runs: Iron, 93·40 per cent; graphitic carbon, 3·12; combined carbon, ·40; silicon, 2·43; phosphorus, ·34; sulphur, ·020; manganese, ·24.

No. 5 iron usually contains ninety-five per cent. iron with about 3·2 per cent. carbon, nearly all combined.

Much can be judged of the quality of iron as it flows from the furnace. Exceptions occur in their fluidity by the crucible cooling it after it is made, as at times No. 1 has been found to flow sluggish and thick. The local causes for changes must be studied carefully by the furnace man, or he is at a loss to account for things taking place, and changes burden or heat in blast, when it would be better to leave it alone. Like the skilful doctor, the founder must study his dumb patient.

DOWNWARD-DRAUGHT FURNACES.

CARLOS A. LOZANO and H. F. T. ERBEN.

(*Concluded from vol. cxxiv, page 388.*)

C. G. S. thermal units per gramme of the coal, 7,607 C. G. S. thermal units per gramme of dry coal, 8,475 C. G. S thermal units per gramme of combustible, 15,255 British thermal units per pound of combustible, the composition of the coal as fed during the two test being respectively—

	Before Change.	*With the Post and Sawyer Attachment.*
Carbon,	·8387	·8417
Hydrogen,	·0150	·0150
Sulphur,	·0093	·0093
Liquid water,	·0385	·0355
Ash,	·0985	·0985
	1·0000	1·0000

whence, the composition of the coal as disposed of by the furnace, was respectively—

	Before Change.	*With the Post and Sawyer Attachment.*
Carbon,	·80251	·77101
Hydrogen,	·01500	·01500
Sulphur,	·00930	·00930
Liquid water,	·03850	·03550
Ash + unburned coal, .0985	·13469	·16919
	1·00000	1·00000

whence the composition of a pound of combustible burned during each test—

	Before Change.	*With the Post and Sawyer Attachment.*
Carbon,	·971	·969
Hydrogen,	·018	·019
Sulphur,	·011	·012
	1·000	1·000

each pound of combustible burned carrying along with it respectively—

	Before Change.	*With the Post and Sawyer Attachment.*
Ash + unburned coal, ... ·119 ·044 ·124 ·089	·163	·213
Liquid water, ...	·047	·045

the calorific power of such combustible as was burned in each case being per pound of combustible burned—

	Before Change.	*With the Post and Sawyer Attachment.*
·971 × 14500 = ...	14080	
·018 × 62032 = ...	1117	
·011 × 3996 = ...	44	
·969 × 14500 = ...		14051
·019 × 62032 = ...		1179
·012 × 3996 = ...		48
British thermal units, ...	15241	15278

if completely peroxidized.

CHIMNEY GASES.

We took three samples of the chimney gases in each test, one just after slicing and firing, one just before firing, one midway between two firings, collecting them by means of a large aspirator and then transferring them under water to glass bottles provided with good stoppers and kept bottom up under water.

For the analysis we used Elliott's apparatus, and as reagents the following solutions—

K O H, 1 in 20 parts of water, for CO_2.

Br, for illuminants.

K O H, 1 in 20 parts of water, with 3 per cent. pyrogallic acid, *i. e.*, potassic pyrogallate, for O.

$Cu_2 Cl_2$, 1 in 4 parts of strong H Cl, for CO,

the analysis being necessarily incomplete for want of the additional apparatus for the estimation of the H and $C H_4$, or facilities for otherwise analyzing the residue of the gases; quite sufficient, though, for the purposes of the investigation.

The analysis was performed in a cool room, at nearly constant temperature, with water and chemicals at the temperature of the room. No traces of illuminants were found in any of the samples.

The results of the analyses are per unit volume—

(I.) Sample taken just after slicing and firing—

	Before Change.	*With the Post and Sawyer Attachment.*
CO_2,	·087	·064
O,	·075	·084
CO,	·010	·012
N + etc.,	·828	·840
	1·000	1·000

(II.) Sample taken just before firing—

CO_2,	·083	·074
O,	·094	·074
CO,	·000	·029
N + etc.,	·823	·823
	1·000	1·000

(III.) Sample taken midway between two firings—

CO_2,	·086	·070
O,	.095	.105
CO.,	.000	·010
N + etc.,	·819	·815
	1·000	1·000

For the reduction to parts by weight we used the following—

Density of H =	1·
CO_2 =	21·94
O =	15·96
CO =	13·96
N =	14·01

Theorizing on the composition of the item N + etc., we got figures which check very well with the above given, thus testing the correctness of the above results.

Multiplying together the volumes and densities, we get—

SAMPLE I, BEFORE CHANGE.

$$
\begin{aligned}
CO_2 &= 1·90878 \text{ containing } O = 1·38820 + C = ·52058 \\
O &= 1·19700 \text{ containing } O = 1·19700 + C \\
CO &= ·13960 \text{ containing } O = ·07977 + C = ·05983 \\
& 3·24538 \qquad\qquad\qquad\quad 2·66497 \qquad\quad ·58041
\end{aligned}
$$

$$\frac{O}{C} = 4·592.$$

SAMPLE I, WITH THE POST AND SAWYER ATTACHMENT.

$$\begin{array}{llll} CO_2 = 1\cdot40416 & \text{containing } O = 1\cdot02121 & + C = \cdot38295 \\ O = 1\cdot34064 & \text{containing } O = 1\cdot34064 & \\ CO = \cdot16752 & \text{containing } O = \cdot09573 & + C = \cdot07179 \\ \hline 2\cdot91232 & \qquad 2\cdot45758 & \qquad \cdot45474 \end{array}$$

$$\frac{O}{C} = 5\cdot404.$$

SAMPLE II, BEFORE CHANGE.

$$\begin{array}{llll} CO_2 = 1\cdot82102 & \text{containing } O = 1\cdot32438 & + C = \cdot49664 \\ O = 1\cdot50024 & \text{containing } O = 1\cdot50024 & \\ CO = 0\cdot00000 & \text{containing } O = 0\cdot00000 & + C = \cdot00000 \\ \hline 3\cdot32126 & \qquad 2\cdot82462 & \qquad \cdot49664 \end{array}$$

$$\frac{O}{C} = 5\cdot687.$$

SAMPLE II, WITH THE POST AND SAWYER ATTACHMENT.

$$\begin{array}{llll} CO_2 = 1\cdot62356 & \text{containing } O = 1\cdot18077 & + C = \cdot44279 \\ O = 1\cdot18104 & \text{containing } O = 1\cdot18104 & \\ CO = \cdot40484 & \text{containing } O = \cdot23134 & + C = \cdot17350 \\ \hline 3\cdot20944 & \qquad 2\cdot59315 & \qquad \cdot61629 \end{array}$$

$$\frac{O}{C} = 4\cdot208.$$

SAMPLE III.—BEFORE CHANGE.

$$\begin{array}{llll} CO_2 = 1\cdot88684 & \text{containing } O = 1\cdot37225 & + C = \cdot51459 \\ O = 1\cdot51620 & \text{containing } O = 1\cdot51620 & \\ CO = 0\cdot00000 & \text{containing } O = 0\cdot00000 & \\ \hline 3\cdot40304 & \qquad 2\cdot88845 & \qquad \cdot51459 \end{array}$$

$$\frac{O}{C} = 5\cdot613.$$

SAMPLE III.—WITH THE POST AND SAWYER ATTACHMENT.

$$\begin{array}{llll} CO_2 = 1\cdot53580 & \text{containing } O = 1\cdot11695 & + C = \cdot41885 \\ O = 1\cdot67580 & \text{containing } O = 1\cdot67580 & \\ CO = \cdot13960 & \text{containing } O = \cdot07977 & + C = \cdot05983 \\ \hline 3\cdot35120 & \qquad 2\cdot87252 & \qquad \cdot47868 \end{array}$$

$$\frac{O}{C} = 6\cdot001.$$

MEAN SUPPLY OF AIR.

Assuming that sample (I.) of the chimney gases represents average sample during five minutes after each firing, and that each of the others is average sample during one-half of the rest of the time, we find the mean $\frac{O}{C}$ in the gases. Thus,

	Before Change.	*With the Post and Sawyer Attachment.*
Mean $\frac{O}{C} = \frac{4·592 \times 115 + (5·687 + 5·613)\ 422·5}{960} =$	5·523	
Mean $\frac{O}{C} = \frac{5·404 \times 145 + (4·208 + 6·001)\ 416}{977} =$		5·148
Multiplying these by the quantities of carbon in the combustible burned—		
$\frac{O}{\text{Combustible}} = ·971 \times 5·523 =$	5·363	
$\frac{O}{\text{Combustible}} = ·969 \times 5·148 =$		4·989
Adding to which the oxygen needed to burn the hydrogen in the one pound of combustible—		
$·018 \times 8 =$	·144	
$·019 \times 8 =$		·152
And the oxygen needed to burn the sulphur in the one pound of combustible—		
$·011 \times 1 =$	·011	
$·012 \times 1 =$		·012
The sum is the		
Total mean $\frac{O}{\text{Combustible}} =$	5·518	5·153
$\frac{100}{23}$ of which is the		
Total mean $\frac{\text{Dry air}}{\text{Combustible}} =$	23·98	22·40
whence,		
Total mean $\frac{\text{Dry air}}{\text{Dry coal}} =$	20·61	18·48
and		
Total mean $\frac{\text{Dry air}}{\text{Coal as fed}} =$	19·82	17·81

HUMIDITY OF THE AIR.

From the observations of the temperatures by dry bulb and wet bulb thermometers, and by Table VII, of the hygrometrical tables by Arnold Guiot for the Smithsonian Institution, we find that the mean pressure of water vapor in the air in the boiler room during the tests was—

	Before Change.	*With the Post and Sawyer Attachment.*
P = in inches of mercury, at 32° F., . . .	1·4901	1·658
And as the mean barometrical reading reduced to 32° F., was	30·05	29·839
The pressure due to dry air was	28·560	28·181

The weight of a cubic foot of dry air, at 32° F., under the pressure of 29·922 inches of mercury is ·080728 pound. The weight of a cubic foot of steam gas, temperature and pressure the same, is ·622 × ·080728 pound. For the given variations of pressure and temperature both air and vapor may be assumed to behave as perfect gases, when, following Balfour-Stewart's reasoning, Art. 158 of his *Treatise on Heat*, the weight of dry air in a cubic foot of the atmospheric mixture was, during the first test—

$$W_a = \cdot080728 \times \frac{28\cdot560}{29\cdot922} \times \frac{1}{1 + (110\cdot6 - 32)\dfrac{\cdot365}{180}}$$

and the weight of vapor of water in the same cubic foot,

$$W_w = \cdot622 \times \cdot080728 \times \frac{1\cdot49}{29\cdot922} \times \frac{1}{1 + (110\cdot6 - 32)\dfrac{\cdot365}{180}}$$

whence the weight of water vapor per unit weight of dry air in the mixture is—

$$\frac{W_w}{W_a} = \cdot622 \times \frac{1\cdot49}{28\cdot56} = \cdot03245$$

and similarly, by substitution of the corresponding figures, for the second test, whence,

	Before Change.	*With the Post and Sawyer Attachment*
$\frac{W_w}{W_a}$ = ...	·03245	·03659

The specific heat of dry air being ·238 and that of steam gas ·475, it follows that the mean specific heat of the atmospheric mixture during the experiments was—

	Before Change.	*With the Post and Sawyer Attachment.*
Specific heat of atmospheric mixture, . . .	·24545	·24636

MEAN LOSS OF HEAT BY FORMATION OF CO.

From the weights given in pages 425, 426, we may, by means of the assumption used in computing the mean supply of air, calculate the carbon which underwent partial oxidation, thus—

	Before Change.	*With the Post and Sawyer Attachment.*
$\dfrac{\dfrac{\cdot 05983}{\cdot 58041} \times 115}{960} \times 971 =$	·012	
$\dfrac{\dfrac{\cdot 07179}{\cdot 45474} \times 145 + \left(\dfrac{\cdot 05983}{\cdot 47868} + \dfrac{\cdot 17350}{\cdot 61629}\right) 416}{977} \times \cdot 969 =$		·19

are respectively the fractions of a pound of carbon oxidized to CO per pound of combustible during each test.

Multiplying these by 10,100, we get the mean loss of heat per pound of combustible in each trial, owing to CO, respectively

	Before Change.	*With the Post and Sawyer Attachment.*
British thermal units, ...	121	1919

Thus by means of the actual imperfection of the combustion in the furnace, the mean calorific power was reduced—

	Before Change.	*With the Post and Sawyer Attachment.*
from the British thermal units (see page 424),	15241	15278
to the actual heat obtainable under the circumstances, British thermal units, . . .	15120	13359

per pound of combustible burned.

CALORIFIC INTENSITY.

From the composition of a pound of combustible (page 423), and from the figures given above, we calculated the products of combustion as follows—

BEFORE CHANGE.

		Pounds.	Oxygen Thus Taken Up.
$CO = \cdot 012 (\frac{4}{3} + 1)$,	=	·028	·016
$CO_2 = \cdot 959 (\frac{8}{3} + 1)$,	=	3·516	2·557
$H_2O = \cdot 018 (8 + 1)$,	=	·162	·144
$SO_2 = \cdot 011 (1 + 1)$,	=	·022	·011
1·000			2·728

and the accompanying moisture and ash,

H_2O, ... = ·047

Ash, ... = ·163

The 23·98 pounds of dry air supplied per pound of combustible carried along 23·98 × ·03245 pound vapor of water, whence H_2O, ... = ·778

Besides this we have the nitrogen that went in with the oxygen used in chemical combination, and the air of dilution—

$$N = \frac{77}{23} \times 2\cdot 728, \quad = 9\cdot 132$$

$$\text{Air} = 23\cdot 98 - \frac{100}{23} \times 2\cdot 728, \quad = 12\cdot 120$$

Total, ... 25·968

per pound of combustible burned.

Similarly

WITH THE POST AND SAWYER ATTACHMENT.

		Pounds.	Oxygen Thus Taken Up.
$CO = \cdot 19 (\frac{4}{3} + 1)$,	=	·443	·253
$CO_2 = \cdot 779 (\frac{8}{3} + 1)$,	=	2·856	2·077
$H_2O = \cdot 019 (8 + 1)$,	=	·171	·152
$SO_2 = \cdot 012 (1 + 1)$,	=	·024	·012
1·000			2·494

and the accompanying moisture and ash,

H_2O, ... = ·045

Ash, ... = ·213

The 22·4 pounds of dry air supplied per pound of combustible carried along 22·4 × ·03659 pound vapor of water, whence H_2O, ... = ·820

Besides this, we have the nitrogen that went in with the oxygen used in chemical combination, and the air of dilution—

$$N = \frac{77}{23} \times 2\cdot 494, \quad = 8\cdot 349$$

$$\text{Air} = 22\cdot 4 - \frac{100}{23} \times 2\cdot 494, \quad = 11.557$$

Total, ... 24·478

per pound of combustible burned.

According to these computations, the supply of air per pound of combustible burned was—

	Before Change.	*With the Post and Sawyer Attachment.*
For combustion,	11·860	10·843
For dilution,	12·120	11·557

The following is the computation of the mean specific heat of the products of combustion and necessary adjuncts which share the heat evolved by a pound of the combustible.

BEFORE CHANGE.

	Weights.	*Specific Heats.*	*Products.*
CO,	·028	·245	·007
CO_2,	3·516	·217	·763
H_2O,	·162		
H_2O,	·047	·475	·469
H_2O,	·778		
SO_2,	·022	·155	·003
Ash,	·163	·200	.033
N,	9·132	·245	2·237
Air,	12·120	·238	2·885
	25·968		6·397

$$\frac{6{\cdot}397}{25{\cdot}968} = {\cdot}246 = \text{mean specific heat required.}$$

WITH THE POST AND SAWYER ATTACHMENT.

	Weights.	*Specific Heats.*	*Products.*
CO,	·443	·245	·109
CO_2,	2·856	·217	·620
H_2O,	·171		
H_2O,	·045	·475	·492
H_2O,	·820		
SO_2,	·024	·155	·004
Ash,	·213	·200	·043
N,	8·349	·245	2·046
Air,	11·557	·238	2·750
	24·478		6·064

$$\frac{6{\cdot}064}{24{\cdot}478} = {\cdot}248 = \text{mean specific heat required.}$$

In both cases, if we followed the rather usual practice of taking the mean specific heat of the products of combustion and adjuncts as equal to the specific heat of air, the difference would be considerable.

As above seen, the number of heat units per degree of rise of temperature in the furnace, or, as often called, the water equivalent of the products of combustion and adjuncts, is for a pound of the combustible in each case.

	Before Change.	With the Post and Sawyer Attachment.
	6·397	6·064
From the British thermal units, mean heat evolved per pound of combustion burned (see page 429), ...	15120	13359
We deduct the latent heat of evaporation of the H_2O of chemical combustion and of the moisture in the coal (·162 + ·047) 966·1 =	202	
(·171 + ·045) 966·1 =		209
Leaving British thermal units, ... mean available heat.	14918	13150

The mean temperature of the products of combustion during the trials (the mean external temperatures having been respectively 65° and 78° F.), was in each trial nearly—

	Before Change.	With the Post and Sawyer Attachment.
$65 + \frac{14918}{6·397} =$	2397°	
$78 + \frac{13150}{6·064} =$		2247°

When with the same supply of air all the carbon is peroxidized, the mean temperature becomes a trifle higher than the above higher temperature. If the coal could be burned without dilution, however, the mean temperature would rise above 4,000° F., and 4 or 5 times as high if the nitrogen and moisture could be kept off. A piece of cast iron placed in the hottest of the fire during the test before the change, actually melted down, and if its fusing point was not lower than that given by Bloxam's work on *Metals*, published by Appleton, in 1882, viz., 2,780° F., it may be an additional evidence of the fact that the intensity of the heat on the burning coals and the mean heat of all the products of combustion are two different things.

SUMMING UP.

	Before Change.	*With the Post and Sawyer Attachment.*
Total heat of combustion of 1 pound of combustible burned + the carbon with it, which escaped unburned, British thermal units, .	15879	16569
Latent heat of evaporation of the H_2O of combination and of the moisture in the coal, per pound of combustible burned, British thermal units, ...	202	209
Available heat of 1 pound of combustible + the carbon with it, which escaped unburned, if the combustion had been complete and perfect, British thermal units, ...	15677	16360
Heat equivalent of the unburned carbon in the ashes of 1 pound combustible burned, British thermal units, ...	638	1291
Heat which failed to be generated, owing to the formation of CO, British thermal units, per pound of combustible burned, . . .	121	1919
Mean available heat obtained, British thermal units, per pound of combustible burned, .	14918	13150
Mean heat imparted to the steam made, British thermal units, per pound of combustible (see p. 383), ...	10813	10334
Considering the efficiency of the boiler complete in its two parts, the efficiency of generation and that of transmission of heat, we have—		
Efficiency of generation—		
$\frac{14,918}{15,677}$ and $\frac{13,150}{16,360}$	·952	·804
Efficiency of transmission—		
$\frac{10,813}{14,918}$ and $\frac{10,334}{13,150}$	·725	·786
The efficiency of the boiler being the product of the two, ...	·690	·632
The temperature corresponding to the pressure of the steam generated was by formula, p. 237 of Rankine's S. E., ...	298°·7	298°·6
The mean of the observed temperature of the flue gases was, ...	340°·	293°·

Of course, these figures being simply the means calculated from the hourly observations, are not such accurate means as might be obtained by the use of instruments automatically recording the pressures and temperatures by continuous curves; but they cannot be very far wrong, and it appears safe to infer from them—

(1.) That the boiler before the change had all the heating surface that could do it good under the other existing circumstances.

(2.) That after the change, it had too much heating surface.

The per cent. of increase of heating surface with the Post and Sawyer attachment was 10·8. The per cent. of increase of efficiency of transmission was 8·4.

The claim of "complete combustion" was completely disproved, the losses by unburned carbon and by production of CO being greater than with the ordinary grate and draught, while the rate of combustion per hour per square foot of grate was practically the same in both cases.

A comparison of the quantities of CO and $\frac{C}{O}$ in the products of combustion, pages 425-426, shows that there is no hope of reducing the quantity of the air of dilution by means of the Post and Sawyer device.

A mere inspection of the dimensions compared, page 377, and results of the tests and calculations, pages 383 and 433–434, will show at once, that, whereas the efficiency of transmission of the heat generated was increased, which we should expect from the considerable increase of heating surface, the Post and Sawyer attachment was fairly beaten in all other possible ways, and that its attachment to the boiler was, in this case, disadvantageous.

It may, however, be an improvement, in some respects, if attached to a boiler which greatly needed an increase of heating surface.

FORBES'S ELECTRIC METERS.

By Prof. Edwin J. Houston.

The necessity for some reliable apparatus for automatically registering the total quantity of electricity supplied to any consumer is well recognized. The rapid growth that has recently taken place in the United States in systems of electric distribution by means of alternating currents, has brought before electricians the necessity for some reliable device for measuring and automatically recording the passage of such currents.

There are in general three effects of an electric current, that may be utilized for the operation of an electric meter, viz., the chemical, the magnetic, and the heating effects.

In a system of alternating currents, the chemical effects are practically absent and cannot therefore be utilized. The magnetic effects have been employed for the operation of alternating current meters, with some little success, but these instruments have not proved as reliable as is desired, since, as generally constructed, their indications depend not alone on the volume of the current that is passing, but also on the number of alternations per second.

The third effect of an electric current, viz., the heating effect, has been happily applied by Prof. Geo. Forbes,* of London, to the operation of a current meter, suitable alike for the measurement of direct and of alternating currents. These current meters, when applied to registering the consumption of electricity for constant currents, possess the advantage over instruments depending for their operation on the chemical decomposition of electrolytes, since they readily permit the passage of the entire current through the measuring apparatus, and thus entirely avoid the very great disadvantages of shunts and variations that necessarily occur in the internal resistance of the decomposing cell.

Prof. Forbes, in his current meter, causes the convection currents, produced by the heating of any fluid substance by means of a coil

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* See elsewhere in this impression of the Journal, a paper by Prof. Geo. Forbes, "On an Electric Current Meter."

of wire traversed by the current to be measured, to move a spiral or wheel, and automatically to record the number of such rotations by the use of suitable mechanism.

The following description of his electric meters is taken mainly from his United States letters-patent, No. 366,824, "For apparatus for measuring electricity," granted July 19, 1887.

The general principles of the invention is thus clearly stated by the inventor in the specification of the above-mentioned letters-patent, viz:

"My method of measuring electric currents is founded upon the fact that an electric current passing through a conductor of electricity generates heat, which heat is carried away from the conductor by the air in contact with it, thus forming convection currents in the air. The velocities of these air currents are proportional to the strength of electric current in the conductor. I measure the strength of electric current by introducing any kind of apparatus suitable for measuring the velocity of air currents. I do not confine myself to the use of atmospheric air. I sometimes use hydrogen or other gases, and sometimes I employ liquids."

As is well known, the heat generated by an electric current flowing through any coil of wire is equal to the product of the square of the current strength that passes through the coil of wire into the resistance of the coil. The heat generated by the electric current creates a motive-force that tends to cause or set up air currents. These air currents will acquire a uniform velocity as soon as the resistance caused by the fluid friction of the air produces an opposing force equal to the motive-force of the heat. Any variation in the amount of the fluid resistance will cause a variation in the velocity of the air currents. In order to obtain high velocity with a small current strength, it is only necessary to decrease the fluid resistance as much as possible. This is accomplished by the inventor by placing the coil of wire, in which the heat is produced, an inch or two above the base of the apparatus. By this expedient the air is subjected to less resistance in passing over the base to reach the conductor.

To ensure uniformity in the velocity of the air currents, as well as to protect the moving mechanism from the influence of extraneous currents, a glass, or other suitable shade, is placed over the apparatus. In some cases the heated air currents are directed

toward the registering apparatus by means of a glass cylinder placed inside the shade immediately above the heating wire coil.

The heating coil is made of a flat coil of ordinary wire, or from strips of metal. The generation of heat at the places where the current passes into and out of the coil, is avoided by increasing the cross-sections of the junctions, and so reducing their resistance. This also has the effect of concentrating the heat on the space occupied by the coil proper.

In some of his later forms of instruments, the inventor employs coils of iron wire. Such coils are liable to injury from rusting, thus varying their resistance. This, however, may be avoided by filling the space inside the glass shade with nitrogen or other gas inert to the metal.

Prof. Forbes has applied the principles of his invention to the case of apparatus suitable for the following purposes, viz:

(1.) For measuring current strength.

(2.) For automatically registering the total quantity of current passing through devices placed in parallel on constant potential mains, for both large and small ranges of current.

(3.) For automatically registering the total quantity of current passing through devices placed in a constant current circuit, *i. e.*, suitable to a system of series distribution for both large and small ranges of current.

The following are among some of the more important of the apparatus described in the specifications of the United States letters-patent already alluded to.

In *Fig. 1* is shown an apparatus designed to measure current strength. It consists, as shown, of a spiral *F*, of paper, cut from a disc and suspended by a silver wire *E*, or by a bifilar suspension, directly above the metal ring *H*, that is heated by the passage of the current. A glass cylinder *S*, supported on the columns *B*, *B*, *B*, is provided for directing the air currents on the paper spiral. A glass shade, *C*, *C*, resting on the wooden base *A*, encloses the entire apparatus and shields the paper spiral from the influence of extraneous currents of air. A clip *D*, on top the glass shade, holds the wire *F*. A pointer *I*, supported by the wire, moves over the graduated dial *M*. The binding posts *K*, *K*, serve to pass the current to be measured through the conductor *H*.

With this apparatus, the spiral will be turned through an angle that is proportional to the square of the electric current.

Instead of the paper spiral, the apparatus shown in *Fig. 2* may be used. In this form a cork A^1, is fitted with four vanes of mica, B^1, B^1, inclined at angles of 45° to the vertical. A glass rod C^1, C^1, passes through the centre of the cork, and bears at its upper end

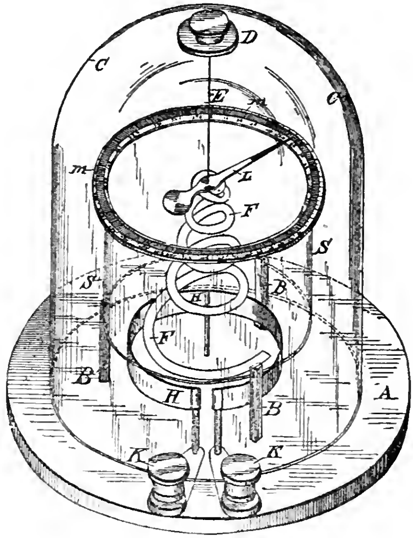


FIG. 1.

the supporting wire D^1, of silver. The pointer E^1, is fixed to the glass rod C^1, in the position shown.

The form of apparatus for measuring the current strength, preferred by the inventor, is shown in *Fig. 3*. Here the portion

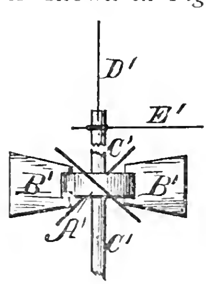


FIG. 2.

rotated by the current is represented by a disc of mica H^2, with the corks fixed at equal distances from each other on its edge. Each of these corks is furnished with a mica vane L^2, inclined at an angle of 45°. This apparatus is supported, as shown, by the fine

silver wire A^2. C^2, is a rod carrying the pointer D^2, which moves on the graduated circle E^2, E^2, in the base of the instrument. The metallic conductor F^2, F^2, which isheated by the current, consists of a circular strip of metal slit at one place, and connected to the binding posts G^2, G^2. An enclosing cylinder of glass is placed as shown.

The graduated circle E^2, E^2, is sometimes divided, "so that the pointer will indicate the current directly, or it may conveniently be divided into 100 parts, so that a deflection of more than one revolution may be noted, a table of values being in this case provided

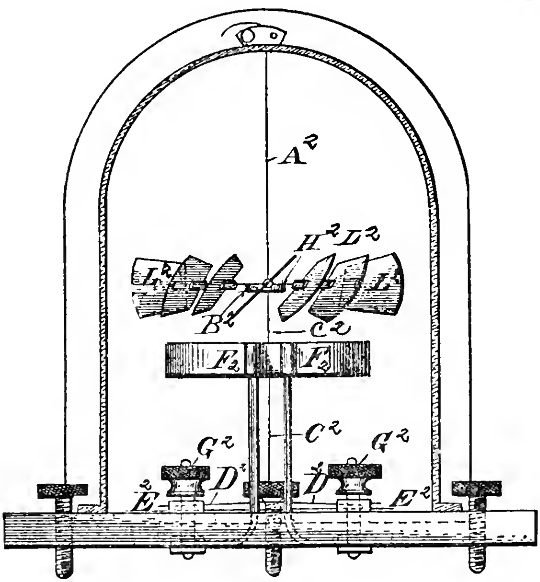


FIG. 3.

for converting the readings into values of the electric current. When it is required to measure very small currents, the conductor may have a high resistance, and may be made of wire wound in many turns."

The high resistance required for this purpose may be obtained, if so desired, by employing in the place of the metallic wire, a string rubbed with powdered plumbago. When any high resistance conductor is used, the instrument may be employed for measuring differences of electric potential.

The inventor thus describes his coloumbmeter, viz.:

"When I wish to measure the total quantity of electricity which has passed through the conductor in any time, I sometimes mount the revolving part upon a delicate spindle, pivoted at the two ends and carrying a pinion or wheel, which gears into wheel work of such proportions as to give convenient means of reading off the number of revolutions of the vanes, as in anemometers, gas meters, etc. The number of revolutions so read off is a measure of the total quantity of electricity which has passed through the conductor. I prefer, however, to use the mica disc and vanes described above, without any enclosing cylinder to direct the air current. In this case, I prefer to pivot the revolving part at only one point. To do this, I cut a circular hole, say one-half an inch in diameter, in the centre of the mica disc, and I fit into the hole a paper cone, about one inch high, and fix it there by shellac." * * * * * "The top of the cone is fixed to a minute ring of aluminium, which is attached to the bottom of a pinion, which is hollow and carries a ruby cup, such as those used for ships or other compasses. This cup rests on a hard steel point fixed vertically on a pillar rising from the base of the apparatus."

An apparatus, such as just described, is shown in section in *Fig. 4* and in plan in *Fig. 5*. The base board, A^3, sustains a brass cylinder, B^3, with a glass top, C^3. D^3, is a metallic base, to which the pillars, E^2, E^2, are attached.

When the metallic base is very heavy, the apparatus will not begin to register until the current has passed through the wire coil for some time. This is due to the absorption of heat by the metallic base. The apparatus will also continue to run for some time after the current ceases. In later forms, the base is made of slate.

The paper cone is shown at e^3, the pinion f^3, at its apex contains a ruby cup which rests on a supporting steel point. The circular disc of mica b^3, has the corks c^3, c^3, with mica vanes d^3, d^3, attached thereto as shown. Any suitable arrangement of wheels and pinions may be employed in connection with the apparatus.

The conductor q^3, may be made of platinum, silver-alloy or other suitable alloy, and is in connection with the binding posts as shown.

"When this apparatus is used as an electric meter in a distribution on the parallel system at constant potential, if there were no

friction in the solid parts of the apparatus, it would act with absolute truth; but besides the air friction, which varies as the square

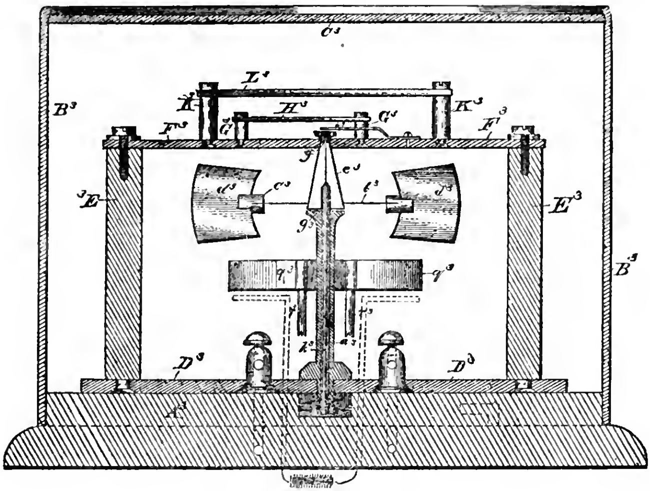


FIG. 4.

of the velocity of the air current, we have the constant friction of the pivot and wheel gearing. This is unimportant when the range

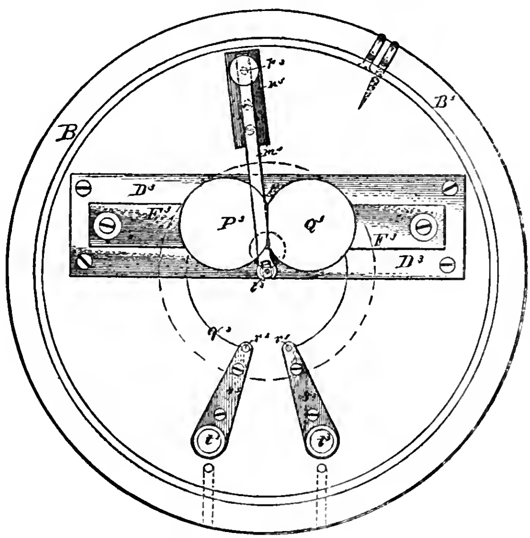


FIG. 5.

of currents to be measured is not large. When this range is large, I introduce a constant source of heat nearly sufficient to overcome

the constant friction, and nearly sufficient to start the apparatus. To obtain this constant source of heat, I employ, besides the main conductor hitherto spoken of, and which is generally of low resistance, a second conductor of high resistance, shown in dotted lines." (See *Fig. 4.*) "I connect the two ends of this conductor with the two wires outside the apparatus, which are at a constant difference of potential, and I arrange the resistance of this second conductor, so that with the constant difference of potentials, the vanes do not quite begin to move. Any current passing through the main conductor will now start the vanes, and by this means the resistance

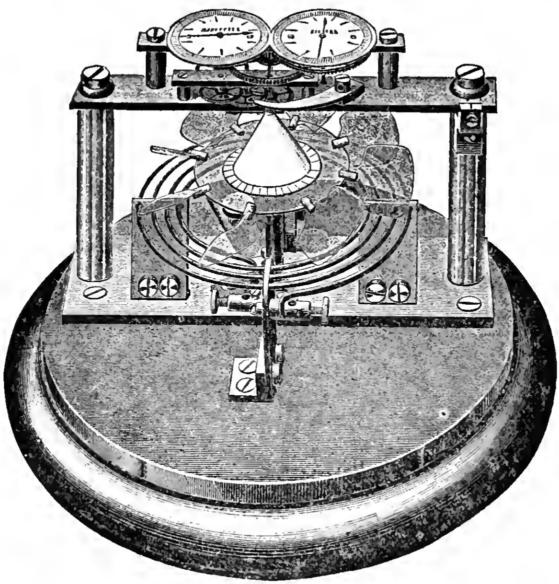


FIG. 6.

of the main conductor may be much reduced, and it will act correctly over a great range of current." The inventor sometimes obtains the constant motive force by means of the current furnished by any suitable battery.

Another form of paper-cone apparatus, differing principally in the arrangement of some of its details is, shown in *Fig. 6.**

When the apparatus is designed to measure the total quantity of electric energy, which passes over a circuit of constant current strength; that is, one in which the electro-receptive devices are in series, the thicker of the two conductors is made of such a low

* For this cut we are indebted to the Am. Inst. of Electrical Engineers, xv.

resistance that the heat from it is barely strong enough to move the vanes. Such a resistance may then be chosen for the fine wire conductor, as will permit the indications of the instrument to include the entire range of potentials to which the system of distribution may be exposed. Where this range is not very great, the heating wire of high resistance is alone employed.

As new types of instrument, the electric meters of Prof. Forbes appear to be entitled to a high rank. More extended use will, in all probability, lead to considerable modifications, without the general principle of utilizing the heating effects of the current its measurement being departed from.

The claims in the United States patent, already referred to appear to be very broad and appear to cover the utilization of air currents for the purpose of measuring the current passing. The following claims will show this to be the case, viz.:

"(1.) In an apparatus for measuring electricity, the combination, with a conductor heated by an electric current, of mechanism for indicating the current of air created by said heated conductor.

"(2.) An apparatus for measuring the strength of the air currents created by the heating effects of an electric current, consisting of vanes placed in proximity to the electric-current conductor, and so supported that they tend to rotate when placed in the fluid current, and one or more fibres, wires, or springs, which resist the rotation of the vanes.

"(3.) The combination, with a conductor heated by an electric current of vanes rotated by the fluid current created thereby, and registering apparatus connected with and operated by said vanes, substantially as set forth."

The other claims are equally broad.

When any valuable invention is brought prominently before the public, it must stand the test of priority as shown by earlier publications. Such anticipations, more or less to the point, generally come rapidly to the front, after the first publication of the new invention. The present invention does not appear to form an exception to the general rule, and already two anticipations have been brought forward. We publish the same without comment, preferring to let them be taken for what they are worth. Other similar publications will doubtless be discovered.

In the issue of the *Electrician*, of London, of October 7, 1887,

Messrs. Jehl & Rupp, of Brunn, in a letter to the editor, dated September 23, 1887, claim to have constructed and tested, as early as June, 1883, a current meter, in which the general principles of construction and operation are not unlike some of the apparatus described by Prof. Forbes. Messrs. Jehl & Rupp do not appear to have published any description of their meter, prior to Prof. Forbes' publication.

In the apparatus, as it is figured in the issue of the *Electrician* for October 7th, before alluded to, the current which is to be measured is passed through a coil of wire in the vertical glass tube *c, c, Fig. 7*, to the left of the figure. This tube communicates, by means

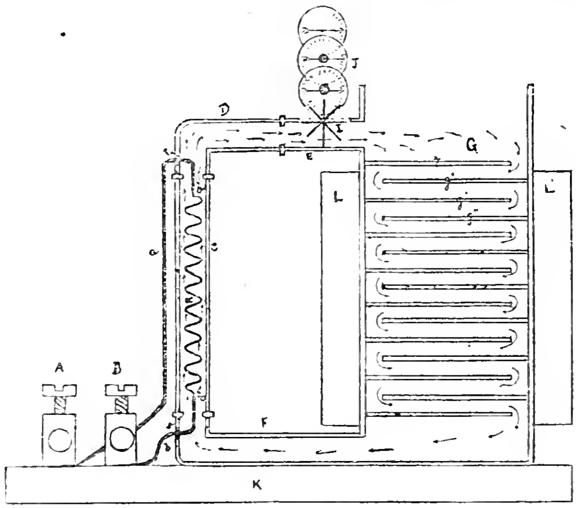


Fig. 7.

of the tubes *D*, and *E*, with a larger vessel *G*, all of which are filled with liquid, preferably olive oil. The currents set up in the liquid on its heating by the passage of the electric current. are caused to rotate a small undershot wheel, which imparts its motion to suitable registering apparatus. The dial faces of this apparatus are shown at *J*.

The larger reservoir *G*, was furnished with square sheets of copper, placed on the sides and alternating with one another. The objects of these sheets was to ensure a complete diffusion of the liquid. To ensure rapidity of loss of heat from the tank *G*, radiating plates were provided, two of which are shown in the figure at *L*, *L*.

The following description is thus given of the apparatus by the inventors in the letter to the *Electrician*, dated September 23, 1887, before referred to.

"The action of the meter was thus: When a current passes through the coil *R*, it heats the liquid at the place, thus causing a circulation, the warm liquid ascending while the cold liquid descends, as shown by the arrows. This circulation causes the undershot wheel to revolve, and its revolutions are registered by the clock work. The stronger the current the more the heat, and thus the more rapid the circulation. The warm liquid once in the tank, which is of a reasonable size, will impart its heat to all the diffusers. The surface of the glass tube, etc., is very small in comparison to the surface of the tank. It will be seen that the function of this apparatus is independent of the outward temperature, for the motion of the liquid is due only to that heat which is generated by the current. When the current does not pass, it is evident that the liquid, at whatever temperature it may be, does not circulate, as all parts are of the same temperature; but the moment the current passes a difference is produced, which causes a circulation in proportion to the current. We may mention that we tried various liquids, and give the preference to pure olive oil. It will also be seen that this meter is good for alternating currents. In conclusion, we may remark that the tests we made gave satisfaction, and we wanted to publish them, but that Mr. Jehl was called away to fit up the Edison exhibit in the Vienna Exhibition, for the Société Électrique Edison, of Paris. After the exhibition, we began our work on the disc machine, and had almost forgotten our meter."

Though we do not for a moment call into question the correctness of the statements of Messrs. Jehl & Rupp, they will, of course, appreciate the fact that the unwritten law for establishing priority of conception of a scientific principle, or the application of such a principle to any scientific apparatus, is decided almost solely by priority of publication. As to what the patent officials in different parts of the world might do for these gentlemen will, of course, depend on the character of the testimony they may bring forward, and the exact nature of the apparatus they actually constructed, as well as the record of the experiments they tried with the same.

Apart, however, from the apparatus of Messrs. Jehl & Rupp, Joseph Tavener took out a patent in England, No. 123, of 1884, for a method of, and apparatus for measuring and registering electric currents, the Provisional Specification of which is dated January 1, 1884.

In this meter, a double-bulbed glass tube (*Fig. 8*), similar in shape to the ordinary differential thermometer, has a wire spiral in each bulb, so connected that the current to be measured can be passed alternately through the wire in each bulb.

The apparatus is poised at its centre of gravity. The current being passed through the wire in one bulb, expands the air, which drives a mercury column into the other bulb. This, now, being

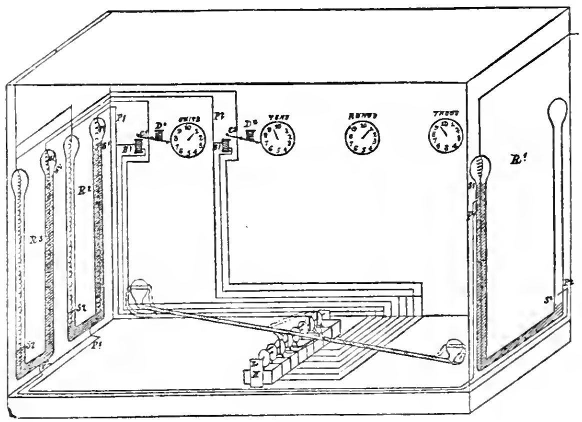


FIG. 8.

weighted, descends, making contacts which diverts the current to the other bulb. An alternate see-saw motion is thus produced, that is measured by suitable counting apparatus.

The principles involved in the operation of the Tavener meter are very clearly put forth by the inventor in his provisional specifications, as follows:

"(1.) A tube terminating in bulbs, similar in principle to a differential air thermometer, but having platinum wire or other substance, of sufficient resistance to generate heat on the passage of a current of electricity through it, inserted through each bulb; the whole being partly filled with a fluid, such as mercury, and hermetically sealed.

"(2.) This tube with bulbs, etc., rests on a support, at a point intermediate between the bulbs, and may be regarded as a hollow lever, whose support is its fulcrum, on which it turns, and whose equilibrium can be disturbed by the displacement of the contained fluid.

"(3.) The whole is so metallically connected up and arranged, that on inserting it in a circuit, the electric current will first traverse one bulb, heating the resistance, expanding the air, driving the fluid to the other bulb, making that heaviest, turning the lever, breaking connection with the one bulb, making connection with the other, heating the resistance contained in that, and in a similar manner again turning the lever—this action continuing so long as the current passes, but its frequency being dependent on the heat generated, which is equal to the product of the electro-motive force into the quantity of current passing.

"(4.) This lever with bulbs is also metallically connected with an electro-magnet in proximity to another lever, upon which the electro-magnet acts, in opposition to a spring, and acted upon by this lever in proximity to the electro-magnet is a train of wheels, whose spindles carry hands, movable over the faces of dials, on which the registrations are to be made, so that at each turn of the lever with bulbs, either connection is made and broken between it, and the electro-magnet, carrying this lever in proximity to it to move through a limited distance, affecting the train of wheels to the same extent, registering at each turn of the lever with bulbs, the same quantity of heat generated by the passage of the current, or its equivalent, the product of the electro-motive force into the quantity of current passing in circuit.

"(5.) Lastly, I particularly point out that the essential feature of my invention is the differential thermometer arrangement, or tube with bulbs, having a resistance inserted through them, and so fixed and placed in a circuit, that the registration of its movements, under the influence of the heat generated by the passage of a current of electricity shall also be a registration of the product of the current into the electro-motive force."

The following figure is taken from the patent specification of the British Patent 123, of 1884, before referred to. Some portions of the figure show apparatus connected with the meter that our space will prevent us referring to in detail.

While describing the operation of electric meters depending for their action on the circulation of currents of fluid caused by differences of temperature, it will be interesting to note a form of electric meter patented in the United States, by Edward W. Weston, November 17, 1885, and numbered 330,451. In this form of meter the liquid currents are not convection currents due to differences of temperature, but are currents or movements of liquid columns traversed by electric currents, and investigated by Lippmann, Dewar, and others.

In the capillary electrometer of Dewar, which might easily be modified for use as an electric meter, a glass tube, supported horizontally, communicates with two dishes containing mercury. The tube is filled with mercury, with the exception of a drop of dilute acid. When a current is sent through the mercury in the tube by placing the terminals of any electric source in the two vessels, a movement of the drop of acid in the tube occurs, the direction of which depends on the direction of the current.

Mr. Weston has utilized some of the phenomena observed by Lippmann, in the construction of an electric meter, the general principles of which will be understood from the following description, taken from the United States letters-patent before referred to, viz.:

"The principle of the construction and operation of the instrument is as follows: If a quantity of mercury be confined in a U-shaped receptacle placed in a magnetic field, and a current of electricity passed through the mercury across the lines of magnetic force, the mercury rises in one arm or part of the U-shaped receptacle and falls in the other, the elevation of the level of the mercury being proportional to the strength of the current."

Mr. Weston's improvements consist mainly in the production of a strong magnetic field, which he claims renders the indications not only more positive, but at the same time more accurate. To obtain this magnetic field, he employs electro-magnets placed on opposite sides of the vessel of mercury. The coils of the electro-magnets are of fine wire, so proportioned that the magnet cores are "practically saturated by any current within the normal working limits of the system with which the meter is to be used. In this way the field of magnetic force will be practically constant and uniform whenever the motor is operating, so

that the record made is a true one of the amount of current that has passed though the instrument and the translating device."

"To utilize this as a meter, it is only necessary to cause the mercury as it rises above a given level in one tube to fall into the meter and measure the quantity thus transferred."

The apparatus is so constructed as to cause the movements of the mercury by the passage of the current, to record the volume passed by a tilting motion, caused by the over-balancing of a tray when a certain quantity of mercury has accumulated thereon. These oscillations are recorded by suitable wheel work.

We believe that this instrument has never been put into actual use.

CENTRAL HIGH SCHOOL.
Philadelphia, November 2, 1887.

AN ELECTRICAL CURRENT METER.

BY PROF. GEORGE FORBES, F.R.S., Etc.

[*Read at the Stated Meeting of the* INSTITUTE, *held Wednesday, Oct. 10, 1887.*]

JOSEPH M. WILSON, President, in the Chair.

THE PRESIDENT.—I beg to introduce to the INSTITUTE, Prof. Geo. Forbes, of England, who will exhibit and explain the Coulomb-meter, a new instrument for electrical measurements.

PROF. GEO. FORBES.—*Mr. Chairman and Gentlemen:* During the whole time that I have been interested in the practical applications of electricity, the subject which has most fully occupied my mind has been the distribution of electricity from central stations. In the period when there was a great excitement about electrical matters in England, some five or six years ago, there were numerous propositions for lighting from central stations, districts and towns. None of these propositions, or very few indeed, were feasible. It has long been known to electricians, as a matter of fact, that it was an impossibility in the earlier stages of electric lighting to supply a great district, or, to too great a distance from the central station. Still, a great deal was possible, and it can be done either by the

old methods by the continuous current up to one-quarter of a mile in an economical way; beyond that, there was no system available. Fortunately we are now possessed of a system which has become thoroughly practical, and which solves the difficulty of lighting at great distances, and that is by alternating currents generated at central stations and of high tension; these currents being transmitted over a thin wire to any distance required. Up to ten miles, the system has been tested already, and the currents are passed through the apparatus, known as secondary generators, transformers and converters, and are reproduced in currents of ordinary tension for the lighting of rooms, such as we have here. This room is being lighted upon that very system.

Now, even when it was thought that possibly we might introduce a direct current to supply power, and supply electricity which flowed through the conductors in the same direction, there was a great difficulty in the way of the introduction of electricity, and that was the lack of a satisfactory meter. But when electricians realized the fact that in future, that is to say, at any rate, until we have a secondary battery, which is cheaper and lighter than anything we have at present—when electricians realized that we shall be using this alternate system of distribution, the fact was forced upon them, that there was absolutely nothing which had been suggested, which could measure the consumption of electricity in one house.

A great many kinds of meters have been devised at different times; all of them, I believe, were intended to be used with continuous currents as distinguished from alternating currents. These may be divided into two classes: the electro-chemical and the electro-magnetic.

The electro-chemical meter depends for its action upon the chemical action which takes place in a liquid when a continuous current is sent through that liquid, and, therefore, is utterly inapplicable to an alternating current, because when we are using electric currents, the direction of the current is being reversed in the circuit hundreds of times every second.

It has been found that all suggested electro-magnetic meters are also unsuitable for alternating currents. The principal objection which has been found with them has been this: that the indications of the meter are not always proportional to the current

that is passing through. It depends partly upon the speed at which the machine is being driven, the rapidity of the alternations, and direction of the currents. Thus, at two rates of speed of the engine, the same current may be flowing into a house and the electro-magnetic meter will indicate totally different supplies of current to the house. Of course, that is not suitable. It is essential that we should have some meter, which is capable of acting the part of the gas meter, only in a more reliable manner.

It is equally important for the consumers and suppliers that such a meter should be provided. It is utterly unsuited to practical people to deal with a general supply of electricity when the charges are made at so much a light per annum; where the supplier does not know whether the consumer is using his light day and night, or only a few hours each night, and when the consumer feels pretty confident that he has a right to light all his lamps the whole twenty-four hours. It is utterly impossible that such a system can last as a basis. The necessity for a meter has forced itself upon everybody.

The chief requisites of a good meter seem to me to be these: It should be accurate within a few per cent. over a very considerable range of current. It should be readable by the consumer as well as by the producer. It should be perfectly simple and as cheap as the gas meter.

It has been found very difficult even in those meters that have been constructed with fair success for the continuous current to comply with the first requisite, that they should be accurate over a considerable range, say from one to twenty lamps, which has generally been beyond the capabilities of accuracy. And in many of the meters which have been constructed to attempt to supply the want of a basis, it has been impossible for the consumer to see at what rate he was using up the electricity.

It was about nine or ten months ago that it was actually forced upon me in a very marked manner, by people who were going to supply the alternating current on an enormous scale, that there really was no meter; that people had been working for it years and years, but had not found it; and it was forced upon me that every effort must be made by persons who were interested in the progress of electric lighting to try to produce such a meter. It seemed to me that any system founded upon the chemical action

of electricity could not succeed with alternating currents. It also seemed that any electro-magnetic machine is certain to give different readings for the same current when the generator of electricity is going fast or going slow.

The next most important electrical action that we are acquainted with is the production of heat, in which the total quantity of electricity used up is wholly converted into this new form of energy; and it seemed to me that if we could devise some simple means of measuring the quantity of heat generated in the conductor by the electric current in the course of so many minutes, we should have the solution of the problem.

And here, also, was a difficulty. The amount of heat created is proportional to the square of the current; thus, if in one experiment I use one ampère of current and in another two ampères of current, the apparatus being the same in each, I am generating four times the heat in one case as in the other. Therefore, the heat evolved is not entirely proportional to the current passed through.

Still, it seemed to me possible to devise an apparatus founded upon the generation of heat which would be accurate. My first experiment was attempted in a very simple way. I cut out a circle of paper, and cut it into a spiral with a pair of scissors, and supported it by a needle from the top of a stick, so that it hung like a spiral worm around the stick. I put an electric wire carrying a current underneath this spiral, and I found that the amount of heat necessary to turn the spiral was exceedingly minute. I tried other sources of heat first, and I found that when the spiral was put under a glass shade, by making a hole in the base of the instrument and putting half an inch of my thumb through the hole, the heat generated was sufficient to make that spiral turn around. You can judge what a very delicate instrument I had.

I was then in communication with a gentlemen, whose name I always like to mention; this is a gentleman connected with Wheatstone in the development of all his extended telegraphic apparatus used by the British Post Office Department: I refer to Mr. Strohm, one of the most perfect mechanicians we have in England. I was in constant communication with him at this time, and he assisted me enormously in the mechanical details of the work. He assisted me not only by his mechanical knowledge, but

by keeping my spirits up when I got despondent, so much so that I felt that he had a great deal to do with the invention; and if it were not that I should commit a German pun, I might call the instrument a Strohmmeter, which would indicate his assistance at the same time. [Laughter].

The instrument which you have before you now is an extremely simple one. A gentleman who saw it in New York, said: "I do not know anything about electricity, but I could have invented that thing very easily. It is ridiculously simple, and it looks as if that is the only form which it would have taken."

I may say it took 10,000 observations and the manufacture of the apparatus in hundreds of different forms before I arrived at what I considered a really practical form of mechanical instrument.

I have a lantern slide which will, perhaps, make it more clear than by drawing attention directly to the instrument, and we can examine the instrument more carefully after the meeting is over. This was taken from a photograph, which gives an extremely accurate idea of the instrument. This (pointing to the instrument on the table) is not the instrument from which the photograph was taken. We have had a great many made, and have had the mechanical arrangements altered to find which was the cheapest to manufacture. Economy is of importance.

The instrument consists essentially of this flat spiral which you see going around in a horizontal plane, consisting of four turns of iron wire. About it there are eight vanes, inclined at an angle of 45° to the horizon, which are attached to small pieces of pith by slots in the pith; and in the same manner, these eight pieces of pith are attached to the circular disc of mica which lies horizontal, which has a hole in its centre, filled up by the base of the thin paper cone. At the top of the cone, there is a jewelled cup, a ruby cup, which is supported on the point of a pin attached to the base of the instrument. The jewelled cup lies in the centre of the pinion which forms the first member of the train of wheel work. The wheel work multiplies ten-fold a good many times over, and finally we are brought to two of the wheels on the arbors of which indexes are attached, pointing to two dials; one dial reads from units to hundreds, and the other reads from 100 to 10,000.

The unit which is adopted is the English Board of Trade unit, which was fixed by the Board for commercial use. It is the

quantity of electricity which passes through a 100-volt lamp using one ampère of current in ten hours. Therefore, if I see an indication of ten units in a night, I would know that 100 ampère hours had been used in the night. If the lamps are not 100-volt lamps, but fifty volts, we would have to multiply by fifty and divide by 100.

You have been wondering what this tongue of metal is which rests just above the first pinion, the jewelled pinion. That is to enable you, by means of a screw at the base, to lift up the whole horizontal mica disc and the cone above it, and the vanes attached to it, and lift it up until it is pressed against the tongue of metal of which I have spoken. That enables you to carry the apparatus about without danger of the parts being injured.

I think these are all the details which I need to show you; in fact, I have explained every part of the instrument. In the instrument before us, the dials differ slightly from the one shown on the screen. The multiplication has been increased 1,000-fold. A worm comes into play attached to the arbor of the most slowly-moving wheel of the train. That worm works in two wheels fixed upon the same axis. One of the wheels has ninety-nine teeth, and the other has 100 teeth, so that in every revolution of these two wheels together, one wheel has gone one hundredth of a revolution more than the other, and the index on one wheel points to a dial fixed on the other wheel; and thus the number of hundreds of revolutions is counted on one wheel, while another index pointing to the first wheel indicates the fractions of a revolution of one of the wheels. One of the dials measures up to 100 revolutions, and the other measures down to one-hundredth of a revolution.

I ought to say, that when I left England I left four instrument-makers in London with instructions to build copies of this instrument slightly modified; my object being to see at what cost each of the different makers would produce them, and the accuracy obtained by different makers. In these new meters I am abolishing the method of counting which I last described, and I am also abolishing the two dials with 100 divisions. I am now introducing four dials like the ordinary gas meters; they will be more commercially satisfactory.

I would like to give you some idea of the range and accuracy of this instrument. In the first place, let me tell you that all instruments which I have had made with smaller conductor and smaller vanes have given me identical results; that is to say, I am perfectly confident that I shall be able to produce meters which will give exactly the same readings. That is a considerable step when we commence to manufacture instruments on a large commercial scale.

I will give you now some instructions as to how these instruments read. I have constructed them generally with the object of using them on a lamp which consumes three-fourths of an ampère. Of course, by modifying the resistance of the conductor, it is perfectly easy to suit them to a lamp which consumes any current we please. I always plot my results down upon a curve, and thus we are enabled to compare one result with another by a glance of the eye.

I measure on a horizontal line the current strengths beginning at 0, and going up to 10 and 20. A meter which was intended to commence indicating at three-fourths ampères, I have used up to twenty amperes with perfect success. That is, we have a fifty-fold range; and I have used my meter either from one lamp to thirty lamps, or ten lamps to 300 lamps.

Measuring the strength of the current on a horizontal line, I measure in a vertical direction the ratio of the current to the speed of the meter. You will easily see that the ratio ought to be perfectly constant if the meter is a good one.

Taking the curve indicated at any point here (on the vertical line) we have a certain rate of speed, one, three, five, six, going say to five. This is a current of two ampères (horizontal). I find as a matter of fact that the instrument begins to start with about half an ampère, but the speed is very small indeed, and therefore the point corresponding to one-half ampère is very high up, and on nearly the same level as the other reading. With three-fourths of an ampère I get a reading of about six or seven. With one ampère I would get my reading about five, and with two ampères it is about five, and with ten ampères it is about five. The curve takes this kind of form: it sinks very rapidly until it comes to about one ampère, and from that point onwards it is quite horizontal until the wire is made red hot. If it is horizontal, it means that the

ratio of the current to the speed is perfectly constant at whatever rate we may be passing the current through the instrument, and that is the condition of a perfect meter. The variations are extremely small.

Yesterday I had a telegram from my assistant in England, in regard to the first of the new batch of meters which is to be the type of all future meters. It states that from the moment the current reached this position (indicating on the diagram), which is one and one-half ampères, it never varied two per cent. from a straight line the whole way up to twenty ampères. I do not mention this, because it is something I have not had before, for my meters have given this result over and over again, but, mark this: I gave instructions to the instrument-maker before I left England; he was left to make the instrument from the design I gave him, and I mention this to show that a good workman is able to produce commercially a meter which involved months of work for me in the production of the first one.

I have shown this meter to assemblies of electricians, one was at the British Association, at Manchester, just before I left England, and the other was at the American Institute of Electrical Engineers, at New York, last week. At both places, a great many questions were asked me, and it will save time now if I answer one: "What influence has the temperature of the room on the meter?"

At first sight you would think this a very serious matter. To begin with, I will answer that, as a matter of fact, I find that it has no effect whatever. It is not difficult to see the reason for this. We are measuring the strength of the convection of currents of air. The strength of the currents depends upon the difference in temperature between the air in the neighborhood of the heated wire, and the air in the neighborhood of the glass. It does not matter whether the air to start with is at zero or 90°, that difference remains the same, and that is what causes the convection of the current of air. The difference remains the same when the wire is heated above the atmosphere to a certain extent. The only influence which the temperature of the room would have would be owing to its heating the wire; this might produce some influence, but not enough to influence the working of the meter to any extent.

Another question I am frequently asked is, whether it would not be better to make a hole in the top and have a free circulation of air through the glass shade. My answer is that it would be somewhat advantageous, but there are objections to the admission of air to a piece of delicate wheel work, and as I have worked until I got the instrument to act thoroughly well in an enclosed space, I think it would be better to let it remain as it is.

I may mention that in this instrument the base to which the wheel work is attached is made of slate. In some I have had it attached to a plate of brass, which got very hot, and absorbed a good deal of heat, and took a long time to cool down at the end of the experiment. By reducing the metallic parts to as small a mass as possible, I cause the instrument to stop very soon after the current has been taken off. In fact, I believe that at present any accumulation of measurement after we have stopped the current is about balanced by the want of measurement when we first put on the current while the wire is getting heated, and the circulation of the air is getting up its swing.

Other questions will no doubt occur to gentlemen here, and I think it best to leave those questions to be asked, and I can answer them when I hear what suggestions are thrown out. I think I shall find now, as I have noted on two previous occasions, that a great many gentlemen here who are very ingenious, and who see the working of the instrument very thoroughly, will make very sound practical suggestions, but in the course of the enormous number of alterations I have made in this instrument, I think I have eliminated all the bad features possible, and reduced it to the most practicable form.

I will now put on a current which I estimated to be some ten ampères. You see the instrument is already going around—(after about ten seconds),—the vanes are now revolving, and if we cared, we could take the speed at which they revolve, but as I have no measuring instrument connected with it, it would be of little interest.

I will now increase the speed by putting on two cells of battery and you will almost immediately see it gradually increasing in speed. It takes a little time for the speed to get up to its full rate. With a very small current of one or two ampères, it takes about two minutes to get up to its full speed, but with the higher cur-

rents it goes very rapidly to its reading. It is now going at a considerably greater speed than when I looked at it last. It is going quicker than it was with a single cell in the ratio of 6 to 10.

We have now in this room illumination by the alternating current system. I forgot to mention that these instruments having been made both for alternating and continuous currents, it was with some interest that I compared the readings of the two currents. I rather expected to find that the self-induction in the wire might cause the wire to have a different apparent resistance with the alternating current from what it had with the continuous current. I have tested this very carefully and find that there is no difference in its standard. With ten ampères direct, it goes with the same speed as with ten ampères continuous.

I will now connect the instrument with the alternating current system, and we will then see what effect we have. You notice that it has stopped. What current have we got?

Prof. Houston.—Three lights.—(Instrument starts slowly.)

Prof. Forbes.—Prof. Houston has kindly arranged the wires, so that I am able to switch on these lamps in the centre of the room, or this one here, which is the equivalent of nine of the other lamps.

We will make a measurement. I am afraid I have not mounted the instrument very delicately on this table, and it does not seem to be going at all with its usual vigor, considering that three ampères are going through it. It has only been on for about a minute, and it requires a few minutes to come up to its full swing. I am afraid there is a sticking point in it. In coming over from New York, I broke one of the vanes off, and I had quite a little trouble in fixing it. There seems to be one point where it sticks and has difficulty in getting over. I am afraid it is that vane. It is possible that the vane I put on this afternoon jams somewhere, as it always sticks in the same position. We will put on the nine ampère lamp and see how it goes. We now have the nine ampère lamp at work on it. I have no doubt, you will soon find it going on all right; even if there is a little dust touching any one of the vanes, we have enough current to drive it. I am certain I shall find a bit of dust, if there is a halting point. Nevertheless, it goes merrily around.

I will take the speed of revolution, when it comes up, to its full swing, and measure the time occupied by ten revolutions of the wheel—thirty-eight seconds.

DISCUSSION.

THE PRESIDENT.—The subject is now open for discussion.

MR. COOPER.—I now repeat the question, whether the instrument works the same under varying conditions of humidity?

PROF. FORBES.—I have never made any special measurements, either for humidity or temperature, but I have observed the same instrument continuously day after day in the laboratory where there was no fire, and where it was subject to all the changes in humidity, and have never noticed any difference in its working.

MR. COOPER.—It seems to me that the surrounding atmosphere, when very absorbent, would be apt to dissipate the heat through the whole volume of air; you have the same current throughout, and the convection currents are not relatively so hot.

PROF. HOUSTON.—Speaking from a practical standpoint, I think Prof. Forbes is to be greatly congratulated for the success he has made in the development of his coulombmeter. Now that successful distribution of electric lighting by means of the alternating current is an assured success, and the areas lighted can be extended almost indefinitely, he has succeeded in removing almost the only difficulty or obstacle in the way of the commercial sale of the electric current. It is true that before Prof. Forbes' coulombmeter was invented there had been meters for direct and alternating currents, but I do not think that any electrician would claim that the meters for alternating currents before the invention of Prof. Forbes's coulombmeter could be considered a very pronounced success.

There are three properties that may be utilized in the measurement of the electric current, viz.: the chemical, the heating and magnetic power. I think Prof. Forbes is perhaps the first who has thought of using the heating power of the current for the measurement of the number of coulombs that pass. He certainly has made a meter that possesses this remarkable property, that it will serve alike for direct or alternating currents, and I believe with the same standard. That latter is a very remarkable achievement in itself.

This coulombmeter, it seems to me, from a practical standpoint, has in it the germ, if not the completion, of a good instrument, because it fulfils those conditions which Prof. Forbes has so clearly laid down, viz; that it shall be accurate, not for a single measurement only, but through a range of form one to thirty or any number of lights; that it shall be understandable by the consumer. If the consumer wishes, he can tell about the amount of energy he is using. It is simplicity itself; a simple mechanism for being moved by convection currents, and a registering apparatus to register the rapidity of the movement.

I can congratulate Prof. Forbes also from another standpoint. I have this evening read his U. S. patents. I have had some experience in patents, and I think he has a base patent; although we do not know what the journals may have in store for us. I think he is the first to use heat in this manner.

I would like to make some correction of the Professor's remarks. He gave too much credit to the good workman who could build the instrument from his instructions. He laid too little stress upon the clear instructions given to the workman. [Applause.] I think this is rather an important point. We once had a discussion on this floor as to the comparative value of pure science and practical science. I remember that pure science was lauded and practical science was looked upon as beneath the dignity of a scientific man. It seems to me that the truest science is what will back its convictions by its money. The truest science is that which can put down on paper just what the scientist claims ought to be, and which will work out, practically, so clearly that by the time, as in this case, the scientific man has crossed 3,000 miles of ocean, he can be advised that an instrument has been constructed, according to his description, which will do practical work. [Applause.]

On motion of Mr. Houston, the thanks of the INSTITUTE were tendered to Prof. Forbes for his valuable paper.

FLASH-LIGHT PHOTOGRAPHY.

By Mr. John Carbutt.

[*Being the Substance of Remarks made at the Stated Meeting of the* Institute, *held Wednesday, October 19, 1887.*]

Jos. M. Wilson, President, in the Chair.

Mr. John Carbutt.—*Mr. President, Ladies and Gentlemen:* I take pleasure in bringing to your attention this evening the latest thing in instantaneous photography at night; although it is not instantaneous, for the duration of the flash is perceptible. It was but recently brought to the notice of the public at the meeting of the Photographic Society, in New York, on Tuesday last. Being a member of that society, I was so far interested in the subject as to go over to see its performance, and was very much struck by it.

While at first I thought the method would be useful only for social entertainment, I can now see where it might be of use in scientific investigations in photographing the interior of mines, geological strata, breasts of coal or in any place where it might be of advantage to have a photographic representation of objects, which could not be reached by daylight.

The invention is the outcome of investigations made by Dr. H. G. Giffard, of New York, who, on reading a description of a patented method of quickly igniting magnesium powder, the invention of a German and patented in England, saw that the material used was of what he considered a dangerous nature, and that it would deter those unacquainted with chemistry from using it. He thought first of using gunpowder to ignite the magnesium powder, but finally decided upon gun-cotton, using the kind which is least explosive in its nature, the variety called collodion, which is not so rapidly explosive as that used for projectiles. The results of his investigations were so satisfactory that, having a little party at my house last week on the occasion of my daughter's birthday, I thought that before they broke up, I would endeavor to secure a picture of them by the new method, it would be a most interesting memento of the occasion, and might be followed by others for a more useful purpose.

Before showing you this light, I will show you some instantaneous pictures, which I took at the University last week of the young athletes in the act of vaulting, and while the pictures are upon the screen I will endeavor to demonstrate the rapidity with which the exposures were made. The first shows the first step made by the champion high jumper, Mr. Page, as he makes his spring before taking the jump.

The second is the five-feet jump. You see him just over the bar. Those who saw this performance will recognize that he turns around and faces the goal from which he starts.

The third is the five-feet-six jump, in which you see that his body is on the turn.

The fourth is a representation of the extraordinary feat of Page breaking his own record by jumping six feet four inches.

The fifth represents Tom Ray vaulting with a pole and clearing the bar at eleven feet four inches. The sixth is the eleven-feet-six jump. In this jump, you will notice that after he rises to about the height of the pole, he climbs up it, hand-over-hand, about three hands, and elevates his feet over the pole and holds on with his left hand until he gets his feet clear, and then throws himself forward and the pole backward, using the momentum to pitch his body over the bar.

This last picture is the group of children, the little party at our house, which we called our "Donkey Party;" it was taken by the flash which will be presently illustrated to you.

It will be readily seen how portable the photographic apparatus is. The apparatus now provided is of such a portable nature that wherever anybody can go with his body he can take it along. I have in my hand six charges. If the President will kindly sit for a moment, I will photograph him.

I have here an extemporized arrangement upon which to burn the magnesium. Here in this paper are twenty grains of pyroxyline, commonly called gun-cotton, and here is a piece of asbestos underneath. I scatter fifteen grains of magnesium powder on the gun-cotton, and by touching the gun-cotton with a lighted taper it will explode with a puff and ignite the magnesium-powder. All now being in readiness, I will put out the lights.

A VOICE—Is your plate in?

MR. CARBUTT—I think you are right, Mr. Chambers. You have been where photographers have been at work before.—(Puts in the plate, fires the preparation, which ignites with a brilliant and diffused flash.)—The exposure is made, and the picture is taken. I will endeavor to show you the results at the next meeting; there are no facilities here for developing.

On motion of Dr. Wahl, the thanks of the INSTITUTE were tendered to Mr. Carbutt for his very interesting illustration.

THE REACTION OF A LIQUID JET;

Being a Review of § 522 and § 523 of Weisbach's „Ingenieur- und Maschinen-Mechanik, Erster Theil; Fünfte verbesserte und vervollständigte Ausgabe, Braunschweig, 1875;"

With some additional matter.

BY PROF. J. BURKITT WEBB, Stevens Institute, Hoboken, N. J.

(*Continued from page 156.*)

The effect of placing $F = G$ is best seen by means of eq. (24), which becomes

$$h' = \frac{c^2}{2g} = \frac{h}{1 - 1} = \infty \qquad (24')$$

and shows that the velocity will be infinite if the water is to be furnished at A fast enough to keep the vessel or pipe full at F while gravity is free to act upon it in its passage from A to F. Eq. (49), which should read

$$V = 2\,F\,h'\,\gamma\,(sin\,a - 1), \qquad (50)$$

gives, therefore, an infinite value for V, as it should. Another form of (24′) is,

$$h' = \frac{c^2}{2g} = \frac{h = 0}{1 - 1} = \text{any value}, \qquad (24'')$$

according to which if A be on the same horizontal line with F (see *Fig. 3*) the pipe between may be of uniform section and remain full; or, taking friction into account, A may be at such a height above F that a column of water of the height $h = A\,F$, will just balance the friction between A and F. The difficulty in supposing $h > 0$ and $F = G$ when there is no friction is that the

descending stream will fall clear of the pipe (as shown in *Fig. 4*) when the distance $A\ F$ is large; if it is small the water may adhere to the pipe enough to keep it full, as is supposed to be the case in *Fig. 2*, above A. The formula, however, does not take into account either friction or adhesion.

From (5) and (7) we obtain for any values of F and G,

$$h = \frac{G^2 - F^2}{F^2} \cdot \frac{c_1^{\,2}}{2\,g} \tag{5'}$$

from which it appears that for h to have a minus value $F > G$, a case shown in *Fig. 5*. The general statement may also be made

Fig: 3.

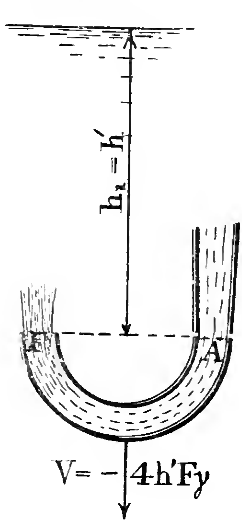


Fig. 4.

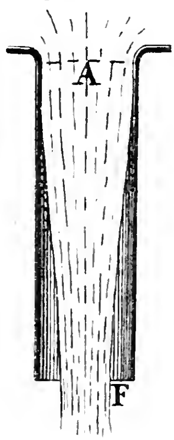
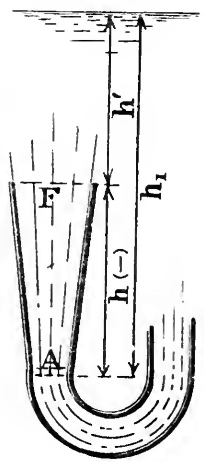
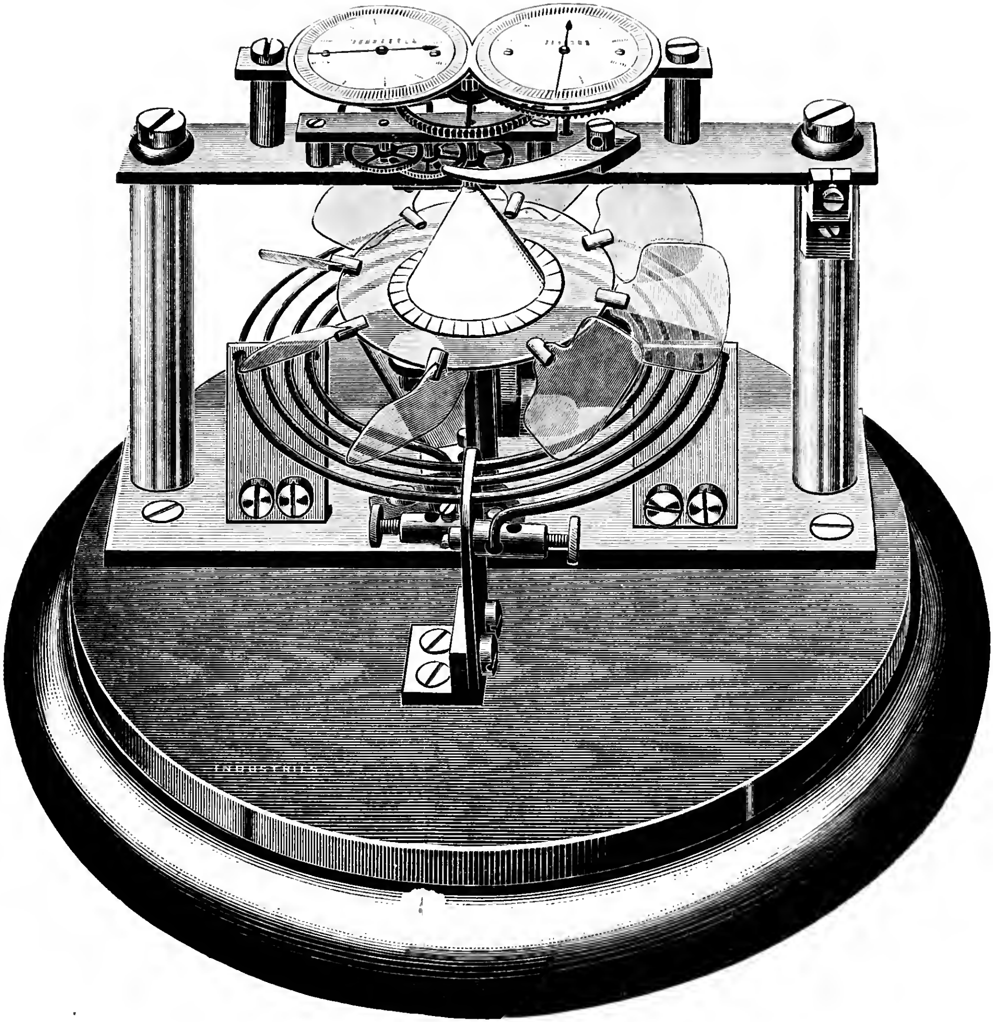


Fig: 5.



that if F be too small for G and h, as determined by eq. (5′), the velocity c_1 will be checked, *i.e.*, the water will not be able to flow away from A as fast as supplied, while if F be made too large the water will cease to fill the pipe.

The last value for V given in (49) is superfluous, and the statement that "V does not act upward, but downward," should be taken as meaning that while in (43) and (44) V will act *either* upward or downward, according as *sin* α is greater or less than $F \div G$, in eq. (50) it will always act downward, because *sin* α cannot be greater than 1.


INDUSTRIES

Equations (51) and (53) should be written

$$H = 0 \text{ and } V = -2\, F\, h'\, \gamma \left(1 + \frac{F}{G}\right) \tag{52}$$

omitting "$= R$" because R is the reaction of the out-flowing jet and not the combined reaction of both, and

$$V = -4\, F\, h'\, \gamma \tag{54}$$

This latter value of V is shown in *Fig. 3*. Eq. (55) should also be written without R, thus;

$$H = 0 \text{ and } V = 2\, F\, h'\, \gamma \left(1 - \frac{F}{G}\right) \tag{56}$$

and (57), obtained by putting $\alpha = 90°$ in (46) and not on the supposition that $F \div G = 0$ in (55) (56), should read

$$V' = 2\, F\, h'\, \gamma \tag{58}$$

It is thus evident that the statement as to the diminution of weight refers to (55) (56) and not to (57) (58), because, in view of eqs. (43) (44) and the distinction pointed out between V' and V, it must be clear that the weight is affected by V rather than by V' alone. Were the surface at A an actual free surface, no water being supplied, so that h, c_1 and c were decreasing, the weight would still be reduced by V. This becomes evident when we consider that the downward pressure on the vessel in *Fig. 924* really arises from the impact of the stream upon the converging sides, as we shall show more clearly later; therefore, if we suppose the supply to be suddenly discontinued the stream through the vessel will not be immediately changed and we shall have no sudden change in the weight, but a gradual one as h, c_1 and c change.

The truth of the above is also seen by considering the section G to be made a differential distance below the free surface, which gives a velocity through G equal to the velocity downward of the free surface.

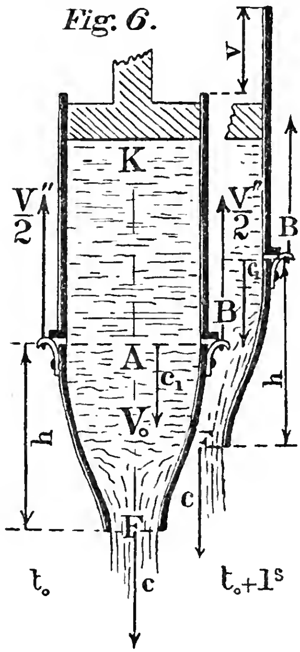
In the American edition (49) is followed by equations for the values of R on the supposition that it is the resultant of H and V, but if R is to be the reaction of the out-flowing jet these should be striken out. Eq. (53) lacks the minus sign; (51) is lacking and (53) seems to have been obtained from the equations for the values of R, above alluded to, the sign being lost on account of a

radical sign. The same ambiguity also exists in regard to h, which should in the latter part of the analysis be changed to h'.

In *Fig. 924*, "*E*" should be *A*.

We will now apply the preceding principles directly to the solution of a simple problem, which, apart from its intrinsic interest, will serve still further to elucidate the method itself.

Fig. 6.



Let the vessel *A F*, *Fig. 6*, be moved vertically upward with the velocity

$$v = c_1 \tag{60}$$

so that we have by eq. (31)

$$c_2 = v - c_1 = 0\,; \tag{62}$$

the supply of water must therefore be at rest with respect to the earth. This will be attained if the air-tight piston K be fixed to the earth and the supply-pipe B moved upward with a velocity v. A small opening, or crack, of constant width, between B and A F, will secure the independent action of A F and insure atmospheric pressure at A; inasmuch, however, as B may be supposed frictionless in sliding past K it might, practically, rest against the vessel

and the whole be thus moved up more conveniently together. Of course, $A\,K$ must be less than thirty-four feet to keep the water up to K.

Equation (5) gives also

$$h = \frac{c^2 - c_1^2}{2\,g} = \frac{c^2 - v^2}{2\,g}, \tag{64}$$

and (7) becomes

$$Q = F\,c = G\,v. \tag{66}$$

Consider now the vessel of water as a separate, or self-contained, system, included between the sections G and F. This system contains the constant amount L_0 of kinetic energy in the flowing water; it is acted upon by the vertical force V'', tending to lift the vessel, and each particle of water is pulled downward by gravity; neglecting the uniform atmospheric pressure on the outside of the vessel and throughout F and G there are no other external forces acting upon it; the system also gives out each second the same amount of water as it takes in but the water is received at rest with reference to the earth and discharged at a velocity $c - v$, thus carrying away from the system the energy per second, see eq (37),

$$L_2 = \frac{c^2 - 2\,c\,v + v^2}{2\,g}\,Q\,\gamma \tag{68}$$

The energy of the system being constant, the work done on it by the external forces must equal the energy discharged, which gives us the equation:

Work done by V'' plus work done by gravity $= L_2$.

The work done by V'' per second, is

$$L_3 = V''\,v. \tag{70}$$

To find the work done by gravity, we must ascertain the height through which each particle descends while under its influence in its passage through the vessel, this height multiplied by the weight of water discharged per second will give the work required. The time taken by a particle to pass through the vessel is the same as that required to empty or fill it; therefore,

$$\text{time from } A \text{ to } F = \frac{V_0}{Q} \tag{72}$$

where V_0 is put for the volume of the vessel. During this time the vessel rises a distance $= v \dfrac{V_0}{Q}$ and the particle descends therefore through the height

$$= h - v \frac{V_0}{Q} \tag{74}$$

with respect to the earth, so that we have for the work done by gravity per second

$$L_4 = Q \gamma \left(h - v \frac{V_0}{Q} \right),$$

which by (64) reduces to

$$L_4 = \frac{c^2 - v^2}{2g} Q \gamma - v V_0 \gamma. \tag{76}$$

Combining these results we have, according to the principle above explained,

$$L_3 + L_4 = L_2$$

or

$$V'' v + \frac{c^2 - v^2}{2g} Q \gamma - v V_0 \gamma = \frac{c^2 - 2cv + v^2}{2g} Q \gamma,$$

which reduces to

$$V'' = V_0 \gamma - \frac{Q \gamma}{g} (c - v). \tag{78}$$

The first term of this value is the weight of the water in the vessel, the weight of the vessel having been neglected, otherwise $- v \times$ weight would have appeared in eq. (76), and consequently the value of V'' would have been increased by the same amount; *i. e.*, it is the weight of the water between the two surfaces at A and F throughout which the pressure is atmospheric. The second term is the momentum of the issuing minus that of the entering water, both taken with respect to the vessel; or, it is the increase of momentum, equivalent also in this case to the momentum of the jet with respect to the earth.

It is to be observed that the supposition made in producing eq. (78) introduces the weight of the water in the vessel, which is not involved in the supposition made in producing the value of L in Weisbach's treatment. *Fig.* 6 shows the apparatus in two positions; first at any time t_0 and then at the time $t_0 + 1^s$, or one second later.

Attention may here be called again to the difference between V', the vertical component of the reaction, R, of the out-flowing jet; V, the algebraic sum of V' and the reaction of the in-flowing jet; and V'', the weight of the vessel of water — V.

The following problem involves the above principles with the exception of the weight of the water.

Suppose a common syringe to be placed in a horizontal position and full of water, which is to be forced out through the nozzle in a horizontal stream; what difference will it make, if any, in the force required whether (a) the barrel of the syringe be held in a fixed place and the piston pushed in, or (b) the handle of the piston be rested against a fixed support and the push applied to the barrel, the velocity with which the water passes through the nozzle to be the same in both cases?

We will now show that a calculation of the pressure against a gradual contraction in a water pipe results in the same value for V as given in (43) or (44), which may be written:

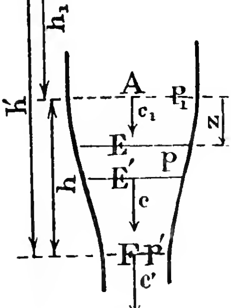


Fig: 7.

$$V = Q\frac{\gamma}{g}\,c \sin \alpha - Q\frac{\gamma}{g}\,c_1 \qquad (44')$$

= outflowing minus inflowing momenta, and becomes for $\alpha = 90°$

$$V = Q\frac{\gamma}{g}\,c - Q\frac{\gamma}{g}\,c_1; \qquad (80)$$

and also in the same value of V'', as defined in the last problem,

$$V'' = V_0\,\gamma - Q\frac{\gamma}{g}\,c + Q\frac{\gamma}{g}\,c_1 \qquad (82)$$

of which (78) is a special case. V'', not shown in *Fig. 7*, is the longitudinal tension in the pipe above the contraction.

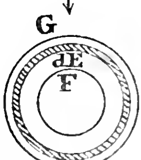


Fig: 8.

Let a vertical water-pipe change by a gradual contraction from a section G at A to a section F at F. The pressure of the water on the non-cylindrical part will have a component P in the direction of the axis of the pipe, which we will calculate using the method of "plane sections." This method takes for an element of

volume the differential volume between two consecutive planes perpendicular to the axis of the pipe and supposes that the velocity and pressure are constant throughout any one such element. These sections may be supposed to be fixed to the pipe or to move along with the water, in which latter case the planes may be supposed to separate from each other, as they move on, just enough to allow for the contraction of the section, and thus keep the differential volume constant. The method is correct only when the contraction is infinitely long, though sufficiently so, as far as practical results might be affected, for all long contractions, it does not therefore lead to a conclusive proof though it furnishes an example of cases where an inaccuracy in the method of proof leads to no error in the particular results obtained.

We shall suppose the planes fixed, they will therefore cut the tapering surface of the pipe into a series of more or less tapering rings, one of which, $E E'$, seen in elevation in *Fig. 7* and in horizontal projection in *Fig. 8*, where it is cross sectioned and marked $d E$. The horizontal projection of the tapering part will be two concentric circles F and G, the space between which will be entirely made up of all the shaded rings $d E$ corresponding with all the rings $E E'$, which make up the tapering part of the pipe. The pressure P, which is to be found, is therefore the sum of all the pressures upon the rings $d E$, so that we may use E as the independent variable and avoid any consideration of the exact shape of the converging part of the pipe.

Evidently the total pressure on the ring $E E'$ equals the surface of the ring multiplied by p, the pressure per square inch of the water against that part of the vessel, and the component in the direction $A F$ of the total pressure equals the above product multiplied by $sin\ a$; the area of $d E$ is, however, equal to the sloping surface of the ring into $sin\ a$, so that the component desired may be expressed as the product of p into $d E$, *i. e.*,

$$d P = p\, d E. \tag{84}$$

By § 427 (Am. ed., § 400,) we have

$$p = p_1 + z \gamma + \frac{c_1^2 - c^2}{2 g} \gamma \tag{86}$$

also

$$p' = p_1 + h \gamma + \frac{c_1^2 - c'^2}{2 g} \gamma \tag{88}$$

from which we get

$$p_1 + \frac{c_1^2}{2g}\gamma = p' + \frac{c'^2}{2g}\gamma - h\gamma, \tag{90}$$

where c has been changed to mean the velocity at any section E distant z below A, p being the corresponding pressure.

The quantity flowing per second will be

$$Q = G\,c_1 = E\,c = F\,c'. \tag{92}$$

Substituting in (84) and integrating we have

$$d\,P = \left(p_1 + \frac{c_1^2}{2g}\gamma\right) d\,E = \frac{c^2}{2\,g}\gamma\,d\,E + z\,\gamma\,d\,E$$

or

$$P = \int_F^G d\,P = p_1\,G - p_1\,F + \frac{c_1^2}{2g}\gamma\,G - \frac{c_1^2}{2g}\gamma\,F$$

$$+ \frac{G^2\,c_1^2}{2\,g}\gamma\frac{1}{G} - \frac{G^2\,c_1^2}{2\,g}\gamma\frac{1}{F} + \gamma\int_F^G z\,d\,E. \tag{94}$$

By applying (90) to the sum of the second and fourth terms we obtain new-second and new-fourth terms and an eighth term, after which we add the third and fifth, the new-fourth and sixth, and the seventh and eight, respectively, and obtain

$$P = p_1\,G - p'\,F + \frac{G\,c_1^2}{g}\gamma - \frac{F\,c'^2}{g}\gamma + \gamma\left(h + \int_F^G z\,d\,E\right) \tag{96}$$

but the quantity in brackets is easily seen to be the volume of the tapering pipe between A and F so that, using also (92), we have

$$P = p_1\,G - p'\,F + Q\frac{\gamma}{g}c_1 - Q\frac{\gamma}{g}c' + W \tag{98}$$

Regarding now the water in the tapering part of the pipe as a separate body or system we may write—

$P =$ downward pressure on system throughout G + reaction of entering jet + weight of system — upward pressure on system throughout F — reaction of issuing jet,

or, calling the entering jet *negative* and the issuing one *positive* and paying attention to the algebraic signs of the pressures, we may write—

$P = \Sigma$ pressures on system throughout jet sections + Σ jet reactions + weight of system. (100)

In the preceding discussion, we are able to neglect the atmospheric pressure because it is uniform over the whole system, which includes the vessel, though the weight of the system is, of course, the weight in air and not in a vacuum; here, however, the pressures at A and F are different and the vessel, with whatever pressure may be outside of it, is excluded from the system. In comparing results allowance must also be made for the change of sign for forces, the upward direction having there been taken as positive, while here plus forces are those in the direction of the flow and of gravity.

P is the pressure of the water downward against the vessel, or converging part of the pipe; $-P$ is, therefore, the pressure which the vessel exerts upon the system. Transposing P in (100) we get $-P$ and can then include it in the first summation and get the equation in the following form—

$$0 = \Sigma \text{ pressures on the system} + \text{weight of system} + \Sigma \text{ jet reactions.} \qquad (100')$$

In the preceding problem, the jet-reactions were excluded from the external forces acting on the system, and they should be so considered, in accordance with the paragraph (tenth) on the term "reaction," because their action is not really at the boundary of the system, but distributed through it; in equation (100), however, they appear in the guise of external forces acting throughout the sections G and F, so that this equation thus viewed, is simply Σ of vertical forces on the system $= 0$.

Let us now compare (98) with (78). By making $c_1 = v$ and $p' = p_1$ and introducing the vessel, with p_1 outside of it, into the system, allowing at the same time for the change in the signs of forces and in the notation, (98) should become identical with (78). The pressure on the outside of the vessel assists V'' in sustaining it against P, therefore we have

$$P = V'' + p_1 (G - F)$$

as the equation of equilibrium of the vertical forces acting on the vessel, P acting downward and the others upward. Substituting in (98) we get, after cancelling terms,

$$V'' = Q \frac{\gamma}{g} c_1 - Q \frac{\gamma}{g} c' + W \qquad (82')$$

which is the same as (82), allowing for the change in notation;

making now $c_1 = v$ the second member becomes equivalent to the second member of (78), which is therefore a special case of (98), as was to be proved.

By omitting both the weight and pressure, we readily see that (98) gives the value for V when $\alpha = 90°$.

It is an interesting result in mechanics that a vessel of water in a condition of "steady flow" weighs the same as if the water were at rest, which is the same, of course, as a steady flow with $v = 0$.

Regarding such a vessel of water as a system we have an example in inorganic mechanics of what is common in organic structures. A plant, or the body of an animal, is a mechanism which preserves its identity, its form, its weight, its energy, etc., while the matter of which it is composed is constantly changing; in the same way the vessel and the water constitute a permanent mechanism which might prove an interesting subject to discuss from a kinematical standpoint. Such a system, or mechanism, might be called a renovated system, or mechanism.

(*To be continued.*)

BOOK NOTICES.

MODERN AMERICAN METHODS OF COPPER-SMELTING. By Edw. D. Peters, Jr., M.D.,M.E., etc, New York: Scientific Publishing Company. 1887.

Dr. Peters has devoted a great part of his busy and useful metallurgical life to the study of copper-smelting, and there is no one better able to speak on that subject than himself. The writer of this review enjoyed three years of his companionship at the School of Mines in Freiberg, Saxony, and has had the pleasure of meeting him afterwards at long intervals at Black Hawk, in Colorado, and at the Ely Copper Mines in Vermont. Wherever Dr. Peters was busy, he was accomplishing much good, and he has usually been busy.

The nucleus of the volume before us appeared in the *Engineering and Mining Journal* some time ago in serial form, as Mr. H. M. Howe's monograph on *Steel* is now appearing in that journal, but it shows every sign of the most careful preparation and intelligent arrangement. Dr. Peters apologizes for not taking up the "Wet Methods" on account of the material addition to the size and cost of the volume which it would entail.

This is the only important want which the volume shows, and is the more emphasized from the fact that the volume is dedicated to Mr. James Douglas, Jr., who, in conjunction with Dr. T. Sterry Hunt, has perfected one of the simplest and best wet processes for the treatment of lean copper ores. But we shall hope that Dr. Peters may be induced later to add a second volume to

his work, which shall be devoted to the wet processes. The first chapter is devoted to some of the localities of copper ores. In this chapter, no attempt has been made to notice the occurrence of copper from the geognostic, but only from the commercial point of view, and hence the native copper occurrences in the orthofelsites of the South Mountain in Pennsylvania, and in the beds of the Merozoic sandstones lying near their junction, not being of commercial significance, are not referred to; though from a geological standpoint they are of great importance. The same is true of the copper deposits in the lower Silurian limestones of Northern Maryland, etc.

Four pages are given to a description of the ores of copper. Twenty pages are dedicated to the methods of copper assaying. Chapter IV devotes forty-five pages to roasting in lump form; Chapter V, twenty-six pages to stall roasting, and Chapter VI, five pages to roasting in lump form in kilns. These three chapters are especially recommended to the copper metallurgist, as they contain a large amount of personal experience of Dr. Peters on a subject to which he has devoted particular attention, and of which the results will not be found elsewhere so concisely and intelligently stated. This is equally true of the following forty-one pages on the calcination of ore and matte in a finely-divided condition; and of the fourteen pages (Chapter VIII), in which the chemistry of this process is treated.

The preliminary ground-work having been finished, a general treatise, in forty-three pages (Chapter IX), considers "Copper Smelting." Thirty pages take up the subject of the brick blast furnace; thirty more are required for the general remarks on smelting in the blast furnace; forty-eight to the reverberatories, and a few pages each are devoted to the "Treatment of Gold- and Silver-bearing Copper Ores," and to "Bessemerizing Copper Matte." Altogether, the book is a valuable addition to metallurgical literature, and will be appreciated both by the student and the practical smelter. P. F.

THE ATOMS OF THE ELEMENTS, OR IRON AND THE OTHER ELEMENTS. By William Coutie. Troy, N. Y.: Author. 1887.

This pamphlet belongs to a class which contemporary science always has difficulty in assigning to its proper place. Either it is a century in advance of modern science in its conclusions, or it is a product of a disordered brain. There is not so much difficulty in treating the methods employed by Mr. Coutie, to establish these startling conclusions, which he says he has arrived at. His English is peculiarly his own as befits such an original work. He says, for example, in his opening sentence: "So much of this is new, that it is difficult to give it a name that will convey what it is, so we name it *double.*" After explaining "why we give it (the title?) as iron and the other elements," he adds: "It really *here* is such, and from this it may be most easily understood, corrected and extended." Whether this means that iron is the "other elements," is left in doubt, but even if it be, how it can be "corrected," or "extended," even "*here*," is a mystery, though not more so than how it can be "understood *here*." "Its relations to the ether *is* that the ultimate atoms of all matter constantly give out from the ether those *acts*, known to us as

light, heat, force, chemical action and gravity. All these are but different appearances of this primitive ethereal atomic action." The "its" above, refers to "this paper."

We are told, without any introduction, that "the first and most efficient part of the calculation is the acid, its weight, nature and *atomic* form."

"The weight of carbon was fixed a century ago *by accident*" * * it is "the critical point of the whole. What fixes the weight of carbon where it is is that in our attempts to think like others we lose the power to think for ourselves." (Note.—Mr. Coutie announces as a new discovery that the *weight* of carbon is 3. Of course, he refers to the weight of enough carbon to fix one standard monad atom, which was clearly explained and named "atom-fixing" by Hoffman over twenty years ago, and known and expounded by Gerhard and others long before that.)

He continues: "Where *this*" (scheme of his ?) "differs most from present practice, is in that it is not preceded by any theory," etc. He might add, nor followed by any rational conclusion. "Where *this*" (again) "covers most ground, is in that * * * all action becomes the result of this one act—the action of the ether on each one hydrogen atom!" ! !

"Carbon and nitrogen differ from the other elements in that they are composed wholly and directly of hydrogen only, and owe their existence exclusively to their atomic forms."

Here follow geometrical groupings of circles, and more tables, after which this sentence occurs: "Next to the existence of hydrogen, the most important fact in chemistry is the existence of the *acid* (HCH), for it comprises one-half the material universe and holds in subjection under its influence nearly all the other half. It is always combined, and *all its combinations are true elements.* That is, the intensity of its action is greater than our present knowledge."

In the midst of this high science we are treated to an excerpt from the Virginia City *Enterprise* on platinum, under the head-line: "A Mysterious Metal."

Two or three pages follow, professing to discuss gold, atomic heat, heat and air, heat and water. Under "atomic heat" we read with joy that "instead of a *total depravity* of heat, we find that bodies in nature become chemically balanced by outside influences and attain the conditions in which we find them." Heat, then, may hope for an ethereal future, undamned. This golden message should be carried to the public on angel's wings. After some pictures of snow crystals, similar to those of a plate published fifty years ago, we learn that "liquid water is an incompressible *solid* with its atoms free to move in all directions." This seems an unjustifiable attempt to rob Ireland of that industry in which she has had a monopoly for so long, by turning out a cheaper home product of bull. The rest of the pamphlet consists of paragraphs of bourgeois type, following captions, such as "Atomic Volume," "Heat and Life," "Cyanogen and Ammonia." The last paragraph under this latter head is so modest and peculiar that it should be repeated.

"For more than fifty years, the subject has been the pastime pleasure of

my life. But now from it I rest content near to my Maker's feet and view secure the work that is done, for now my day is spent—the goal is reached, the labor is over, the journey is passed, and the victory is won. For now the power that made, moves and maintains the material universe, is opened to human view. Never to close again while truth and manhood are admired among men. The performance of this work will soon become *the brightest jewel in the diadem that crowns the intelligence of North America*, and such it will remain when all that fills its place is passed away and is forgotten. For this is the work of Him whose will we all obey. This is the power by which He called the Sun into being, and this will be His law supreme, where (*sic*) the Sun's race is run, and His work is done and time lays him to rest in never-ending night."

With the startling theology of the last sentence, this imperfect review may terminate without further comment. P. F.

THE METALLURGY OF SILVER, GOLD AND MERCURY IN THE UNITED STATES. By Thomas Egleston, LL D., Prof. of Mineralogy and Metallurgy, School of Mines, Columbia College. In two volumes. Vol. I. John Wiley & Sons. For sale by J. B. Lippincott & Co., Philadelphia.

This work is without any question, in appearance, the largest and most comprehensive that has been attempted in this country, and Prof. Egleston ought to be in many respects better fitted to undertake it than any other. Of course, a book which gives the history of the metallurgical industry, as well as a statement of the processes now in operation, must be largely a compilation, and its value depends greatly on the judgment with which it is compiled. To hold the balance equally between the slower and more accurate processes of the eastern hemisphere and the more rapid and practical methods of the western, requires no little care.

Besides this, what would be good practice in 1860 in one of the remote furnaces of the West, would be very bad practice in 1887, because the commercial side of the problem has been so greatly altered in the intervening time by the change in the surroundings of each furnace, and the difference in the cost of transportation and ways.

It is due to Prof. Egleston to say that he has stated the advantages and disadvantages of the various processes which he cites with commendable impartiality ; with a perceptible tendency, however, to recognize the ultimate superiority of the more thorough and scientific method, whether American or not, and whether it is popular among smelters or not. The volume before us gives in an introduction a sketch of the discovery of gold, and a sketch of placer mining. Following this, is a treatise on hydraulic mining. The work commences with the sub-chapter on the treatment of gold quartz, as it would seem that the line between the mechanical and metallurgical treatment of gold is hard to draw. The only three picture illustrations of the volume are in this part. They are coarsely done, though perhaps they illustrate the subject well enough for a volume of this kind. Chapter II is on zinc desilverization ; Chapter III is on the separation of gold and silver from copper; Chapter IV is on crushing machinery ; Chapter V is on roasting silver ores;

Chapter VI is on the Patio and Cazo processes; Chapter VII is on barrel amalgamation; Chapter VIII is on pan amalgamation; Chapter IX, the treatment of silver tailings; Chapter X, leaching processes.

Each of these chapters should have been an embodiment of a great deal of experience and travel of the author, as well as a generalization which his habits as a teacher might have enabled him to add to the bare statistics. Those things which open to criticism in the book are the matter and the facts, as well as the arrangement, and in some places, the methods of expression.

The title on the cover is open to objection on the ground of English construction—"The Metallurgy of Silver, Gold and Mercury *of* the United States." The object of the investigations is stated to be "for the sake of ascertaining what the real state of the facts *were*." Again: "The spirit of to-day is to recognize no difficulty which, in the *face* of transportation, fuel and water, may not be overcome." "With the limited experience so far, it appears that the value of the mercury lost in amalgamation very much exceeds the expense of waste of chemicals in the Russell process, which are not injurious to health, while mercury is, and allows, at the same time, the extraction of lead and copper as secondary products, which are entirely lost by amalgamation."

The syntax of the above sentences and others throughout the book, is defective, and the meaning of the sentences obscured; but the work is, nevertheless, an imposing and thick contribution to American metallurgical literature. P. F.

THE SUTRO TUNNEL COMPANY AND THE SUTRO TUNNEL; PROPERTY, INCOME, PROSPECTS AND PENDING LITIGATION. Report to the Stockholders. By Theodore Sutro, Attorney, Counsellor, etc., for Sutro Tunnel Company. New York: July, 1887.

This is a neatly-printed work, 8vo, of 185 pages, giving a general account of the great Sutro Tunnel from the time of its commencement, on October 19, 1869, to the present time, when its financial condition seems to be sufficiently desperate, though better than it was. There is nothing of scientific value in the book, the extracts from the testimony of experts being introduced as a lawyer would naturally introduce them—to justify certain points in the chain of narrative of a great law-suit. It will be interesting to the young lawyer and to the general public rather as giving the doubts, fears and struggles of an attorney to overcome obstacles in the way of the success of his clients, and show what may be done in the way of the postponement of decision by the Courts, change of the direction of litigation, and victory over property-wrecking foreign plutocrats by an indefatigable lawyer. P. F.

SCIENTIFIC NOTES AND COMMENTS.

CHEMISTRY.

ANIMAL TANNIN. M. Villon, *Bull. de la Soc. des Elèves* de M. Fremy, through *Chemical News*.—Hitherto there have been known only vegetable tannin with its 800 varieties, and synthetic tannin or digallic acid, not to speak of the so-called mineral tannin, obtained from coal, peat, etc. The author has for a long time sought for an animal tannin, *i. e.*, one formed in the organism of an animal, and appearing either in its secretions or in its tissues. Having been engaged with the weevils, well-known from the damages which they occasion in granaries, he sought for tannin, knowing that, as far back as 1810, M. Penunt, of Lyon, had found gallic acid in these insects.

The corn weevil (*Calandra granaria*) is a coleopterous insect of about three millimetres in length, and of a maroon-brown color. These insects were first killed by exposure to the fumes of chloroform, then ground up and digested for an hour in boiling alcohol at 90 per cent. The extract is decanted into a retort and evaporated on the water-bath to a syrup, and finally dried in a capsule.

The residue is dissolved in acetic ether at 50°; the liquid is filtered, diluted with half its volume of water, and precipitated by means of ammoniacal zinc acetate prepared by dissolving zinc acetate in liquid ammonia diluted with half its volume of water. The first and last portions of the precipitate are rejected.

The precipitate thus obtained consists of zinc tannate, other substances—such as gallic, ellagic acids, phlobaphene, albuminoids, and pectic principles—remaining in solution.

The precipitate is then suspended in water, to which oxalic acid has been added sufficient to throw down all zinc. The mixture is heated nearly to a boil whilst stirring. The tannin remains in solution; the liquid is evaporated to a syrup in a vacuum at a temperature of 50° to 60°. Five hundred parts of weevils yielded fifteen parts of pure tannin.

Animal tannin (fracticornitannic acid) appears in the form of small reddish-yellow scales, soluble in water, aqueous ether, alcohol, chloroform, etc. It possesses all the general properties of the tannins, precipitating gelatin, albumen, and dissolved caseine, and being absorbed by animal tissues.

It precipitates the natural and artificial alkaloids, and gives a bluish-black coloration with ferric salts. Its analysis leads to the formula $C_{28}\,H_{16}\,H_{16}$. Sulphuric acid splits it up into glucose, gallic acid, and an amorphous red product analogous to phlobaphene. H. T.

ANALYSES OF SOME TANNIN MATTERS. (P. Kay and E. Bastow, *Journal of the Society of Dyers and Colourists*, **3**, 9.)—The following analyses of some of the tannin materials, exhibited in the Indian and Colonial Exhibition, 1886, were done during the Session 1886–87. All the estimates were carried out in duplicate by each of us, working independently, by Proctor's modification of Löwenthal's method. The percentages of tannin are given below in terms

of oxalic acid on the one hand, and in terms of gallotannic on the other, each figure representing the mean of four separate estimations:

No.	Name.	Percentage of Tannin	
		In Terms of Oxalic Acid.	As Gallotannic Acid.
1	Terminalia tomentosa (galls),	9·24	6·53
2	" belerica (fruit),	12·86	8·48
3	" chebula "	52·65	34·49
4	Ceriops Roxburghiana (bark),	37·65	24·66
5	Camia auriculata,	19·94	12·86
6	Acacia catechu (extract),	76·00	...
7	" " (bark),	21·35	...
8	" arabica (pods),	22·44	...
9	Areca catechu (nuts),	14·28	...

The percentages of gallotannic acid are calculated from those of oxalic acid by Neubauer's equivalent. H. T.

FREE THIOCYANIC ACID.—Peter Klason (*Jour. Prakt. Chemie*, **35**, 400,) has succeeded in preparing thiocyanic acid by distilling in vacuo a ten per cent. aqueous solution of the acid, drying the vapor by passage over calcium chloride and then condensing in a freezing mixture. The condensed liquid is pale yellow, and soon after the receiver containing it is taken from the cooling mixture, solidifies to an amorphous yellow mass. If a few drops of the still liquid substance be placed in a watch glass, they disappear as rapidly as ether, the anhydrous acid being very volatile and having a penetrating odor. In its behavior it closely resembles free cyanic acid; at low temperatures both are volatile, strongly odorous liquids that become polymerized at a higher temperature, at the same time disengaging heat. When perfectly dry, both appear to be imido-compounds, but in solution they exist as oxy-compounds. A dilute aqueous solution of thiocyanic acid may be most easily prepared by the action of sulphuric acid on barium thiocyanate. W. H. G.

THE REACTION OF AMMONIA ON BLEACHING POWDER, AND THE CONSTITUTION OF CHLORINATED LIME. G. Lunge and R. Schoch (*Berl. Ber*, **20**, 1474).—The authors point out that the reaction between ammonia and chlorinated lime as explained by Dreyfus requires the co-existence of calcium hydroxide and ammonium chloride as products, an evident impossibility. Analytical experiments show that, as had been advanced by Kolb, the products of the reaction are calcium chloride, water and nitrogen. The objections to the formula $Ca\,Cl^2\,O$ for chlorinated lime, which were advanced by Dreyfus, are therefore unfounded. Kolb proposed the use of ammonia as an antichlor, but the destruction of traces of chlorine by ammonia is complete only after several days in the cold, unless an excess of thirty times the absolutely required quantity of ammonia be employed. Since the actual quantity of bleaching powder that remains to be destroyed is exceedingly small, even thirty times the required quantity of ammonia fortunately represents but a small absolute quantity. W. H. G.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, November 16, 1887.*]

HALL OF THE INSTITUTE, NOVEMBER 16, 1887.

JOSEPH M. WILSON, President, in the Chair.

Present, 152 members and forty visitors.

Additions to membership since last meeting, thirty-three.

The Special Committee on Government Scientific and Engineering Bureaux made a report of progress.

Mr. JOHN L. GILL, Jr., read a paper on "Screw-Threads," in which he discussed the comparative merits of the ordinary V, the Whitworth, and the Sellers thread, and called attention to certain alleged deficiencies of the existing systems. He proposed as a substitute, a thread having one side 90° and the other 60°, flat at top and bottom, with eight threads to the inch, and two-eighths taken from top and bottom. He presented the results of a series of tests of the strength of this form of thread as compared with the other forms, which were favorable to the new thread, and exhibited the tested specimens. The paper has been referred to the Committee on Publications.

Mr. FRED. E. IVES made some remarks upon the so-called Japanese Magic Mirrors. He reviewed the explanations that had been offered to explain the curious property which they exhibit, and offered what he believed to be the proper explanation. Referred for publication.

Mr. JOHN CARBUTT exhibited a number of photographs made with the "flash light" mixture referred to in the proceedings of the October meeting.

Mr. CYRUS CHAMBERS, Jr., exhibited and described some improvements to the portable photographic camera which he had devised, and which add to its convenience. They embraced an adjustable tripod, an instantaneous and "time" shutter, and a collapsing focusing-hood.

Prof. EDWIN J. HOUSTON spoke in general terms of the attempts that had been made to devise apparatus for recording and reproducing speech, referring specially to the "phonograph" of Mr. Edison, and to the published statements respecting alleged improvements that the inventor had succeeded in making, and which would make it a useful apparatus. He also referred to the "gramophone," devised by Mr. Emile Berliner, for accomplishing the same objects, and described the instrument with the aid of a picture of the same, projected on the screen.

Adjourned. WM. H. WAHL, *Secretary.*

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PENNSYLVANIA STATE WEATHER SERVICE.

OCTOBER, 1887.

SUMMARY OF METEOROLOGICAL REPORTS

MADE BY VOLUNTARY OBSERVERS,

—TO THE—

METEOROLOGICAL COMMITTEE OF THE

FRANKLIN INSTITUTE,

PHILADELPHIA, PA.

T. F. TOWNSEND, Sgt. Signal Corps, Assistant.

PENNSYLVANIA STATE WEATHER SERVICE BULLETIN FOR OCTOBER, 1887.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, November 1, 1887.

It will be perceived, on examination of the annexed monthly summary, that the number of observing stations has been increased to thirty-two this month, against twenty for the last, and that the summary now includes barometrical observations in certain localities.

As a general thing, the records appear to be carefully made, and it would tend greatly to the accuracy of the results if each observer would add up his columns and calculate the means. These calculations should be repeated and checked at the general office.

Several of the observers attach interesting and instructive notes of the condition of the crops as affected by the weather, and also of special phenomena. That these notes are valuable as a permanent record, will appear hereafter and it is hoped that all of the observers may give them as fully as the space on blanks will admit. A review by each observer of the general results at his station for the month will add to the value of his report.

Observers will find it interesting to compare their results with those of other stations, and thus to establish their relative portion for each month as respects the principal features of the weather.

Observers are particularly requested to give all the data called for on the forms, and to complete and mail them promptly, so that the publication may not be delayed.

The Committee repeat the expression of the hope that the people of each county will organize for the purpose of flagging their county so as to convey promptly to all the farmers, the daily forecasts of the weather for the next twenty-four hours. A little reflection will show the necessity for this. The repeating stations do not repeat the signal for their own benefit, but for that of the people beyond them, who, in turn, pass their signals to their more distant neighbors.

Where some do the work and others reap the benefit, a system of proper compensation is necessary, and this can only be attained by organization.

For the Committee on Meteorology of the FRANKLIN INSTITUTE,
W. P. TATHAM, *Chairman.*

ER SERVICE FOR SEPTEMBER, 1887.

County.	Precipitation.			Wind.	Number of Days.		Observers.
	Greatest in 24 Hours.	Date.	Number of Days Rainfall.	Prevailing Direction.	Cloudy.	Frost Occurred.	
Allegheny, . . .	.20	10	9	N	11	7	Oscar D. Stewart, Sgt. Sig. Corps.
Bedford, ...	.17	. . .	6	. . .	. . .	. . .	Rev. A. Thos. G. Apple.
Bradford, . . .	.57	9	8	NW, W	23	11	Charles Beecher.
Bucks, ...	.45	20	12	NE, W	10	10	J. L. Heacock.
Bucks, ...	2.33	20	9	SW, NW	10	2	Milnor Gillingham.
Cameron, . .	. . .	. . .	. . .	. . .	. . .	. . .	. . . .
Centre, ...	.18	21	4	S	. . .	4	L. Ray Morgan.
Centre, . . .	.14	3	8	W	11	3	Prof. Wm. Frear.
Chester, . . .	.79	20	12	. . .	6	. . .	Jessie C. Green, D.D.S.
Clarion, . . .	.32	10	10	SW	12	4	J. H. Apple, A.B.
Columbia, . . .	. . .	. . .	. . .	. . .	. . .	. . .	Wm. G. Yetter.
Crawford, . . .	. . .	.	. . .	SW	0	. . .	Prof. J. H. Montgomery.
Cumberland, . .	.38	21	5	W	5	4	J. E. Pague.
Delaware, . . .	.74	20	5	NW	13	. . .	Prof. Susan J. Cunningham.
Elk, ...	.60	3	2	. . .	. . .	. . .	Joe Messinger.
Erie, ...	. . .	. . .	17	NW	10	3	Peter Wood, Sgt. Sig. Corps.
Fayette,* . . .	.90	13	6	NW	8	9	Wm. Hunt.
Fulton, ...	.16	27	9	W, NW	9	5	Thomas F. Sloan.
Huntingdon,*	.16	11	5	W	10	8	Prof. W. J. Swigart.
Indiana, . .	. . .	. . .	8	N	13	9	Prof. Albert E Maltby.
Lackawanna,†	.41	11	9	NW	9	5	T. F. Heebner, M.D.
Luzerne, . . .	.95	. . .	10	. . .	12	. . .	H. D. Miller, M.D.
Lycoming,‡ . .	.35	1	6	W	14	6	E. H. Baker.
Mercer,§ . . .	. . .	. . .	5	. . .	8	11	Prof S. H. Miller.
Montgomery, . .	.85	21	5	NW, W	. . .	4	Charles Moore, D.D.S.
Northampton, .	.84	20	6	NW	5	5	Lerch & Rice.
Philadelphia, . .	1.49	20, 21	9	W	13	3	Luther M. Dey, Sgt. Sig. Corps.
Pike, ...	1.20	11	6	S	16	. . .	John Grathwohl.
Schuylkill, . . .	.32	20	7	. . .	. . .	. . .	E. C Wagner.
Somerset, . .	. . .	. . .	. . .	. . .	. . .	. . .	. . . .
Tioga, ...	1.16	. . .	10	SW	22	13	H. D. Derring.
Washington,*	. . .	. . .	4	. . .	. . .	6	Harrison Otis.
Wayne, . . .	.26	1	10	. . .	. . .	13	Theodore Day.
Westmoreland,*	.35	10	9	NW	11	9	H. S. Brunot.

* 3d to 31st, 2(

T. F. TOWNSEND,
Sergeant Signal Corps, Assistant.

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REMARKS.

The general climate of the State for October has been from 2° to 5° colder than usual, especially in the western and elevated counties. The cold of the 15th, 22d, 26th and 31st was quite general, and nearly all the western and northern counties report snow on the 21st and 22d, in amounts from "ground covered" to one inch at Scranton and Wellsboro, and four inches at Greenville. Most vegetation was cut off throughout the State by the killing frost of September 26th, although in some of the Eastern and Southern counties, and in the lower valleys, the hardier sorts were continued until the severe frosts, which occurred during the latter half of October, and from which no district was free. An abundant growth of fall wheat and grass has been reported

The colder belt of highlands had a mean of 45° at observed stations, and about 40° for the higher surfaces. The central counties had an average of 47° at observed stations, with a very considerable area of 50° in the valleys, and of 52° at West Chester, Swarthmore, Uniontown, Pottstown and Carlisle, 53° at Pittsburgh, and 55°·6 at Philadelphia, which is not a full degree below the average for a series of years. The mean at Erie was 48° 9, and therefore 4°·4 below the general mean of 53°·3.

The season at the Lake Shore was probably not so much extended as usual, owing to the severe frosts in September and October. None of these changes were either caused or attended by general storms of severity. The month was remarkably free from storms or floods, the rainfall being generally light. The southwestern part of the State is reported as very dry. In Westmoreland County the scarcity of water in wells and cisterns is causing much inconvenience. At several posts of observation, the rainfall was less than an inch, as Pittsburgh, State College, Ridgway, McConnellsburg, Huntingdon, Indiana, Greenville, Washington, Greensburg, Catawissa, Charlesville, and Phillipsburg. Erie and Fallsington were exceptional with 4·43 inches, at Erie and 3·06 at Fallsington. The number of rainy days varied from four to seventeen, the average for the State being seven. A severe wind storm from the southwest and west occurred in Greensburg, Greenville, Clarionville, Indiana, Meadville and Scranton on the evening of the 23d. The general atmospheric movement for the month was rather less marked than usual, and no northeast storm occurred east of the Alleghany's, as often happens in October.

Lunar halos were observed at Quakertown on the 1st, Indiana 27th, Clarion 27th and 28th. Wild geese were observed flying south on 22d and 26th at Quakertown, 25th at Doylestown and Dyberry, and on the 27th at Greensburg. The observer at Quakertown reports plum trees in blossom on the 14th, and apple trees on the 25th, in protected situations. The ground was frozen at most stations on the morning of the 31st. T. F. T.